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## CHARLES HENRY HULL

*Treasurer of the American Economic Association, 1896-99*

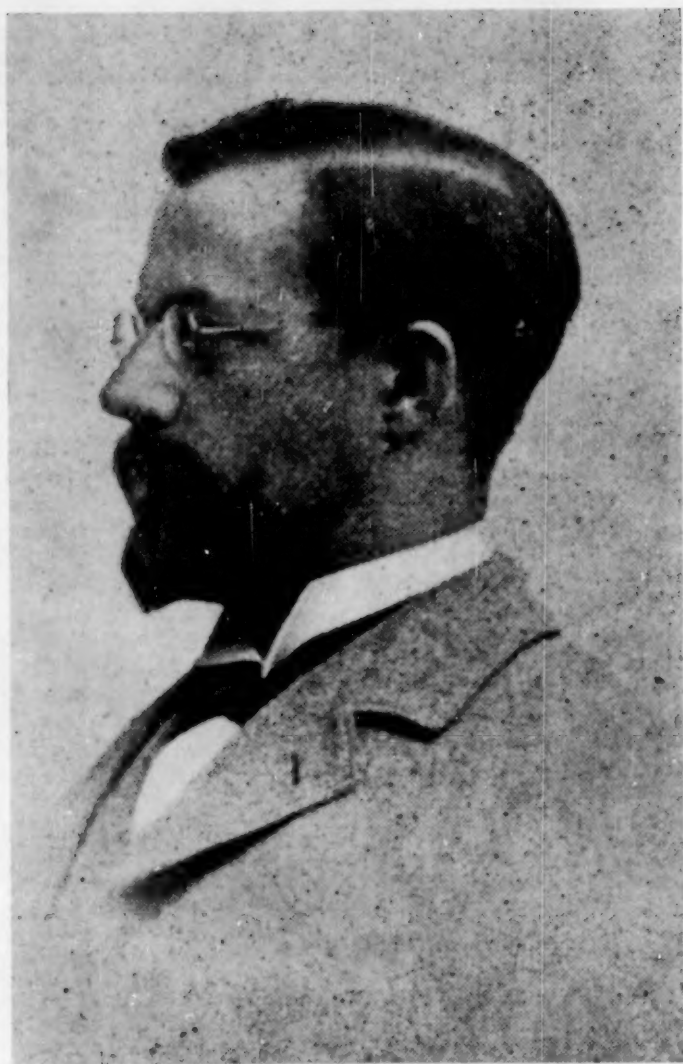
*Secretary-Treasurer, 1900 to June, 1901*

Charles Henry Hull was born in Ithaca, New York, September 29, 1864. His life was devoted very largely to the activities of the city of Ithaca and Cornell University. He received his Ph.B. degree at Cornell in 1886 and except for two years, 1890-92, when he was a student at Göttingen, Halle, and Berlin, he spent his entire academic career at Cornell. He received his Ph.D. degree at Halle in 1892.

Hull's first appointment was in the Cornell University Library in 1886 and he became assistant librarian in 1887. After returning from abroad in 1892, he became instructor in political science and in 1893 was promoted to assistant professor of political economy, which post he held to 1901, when he was made professor of American history, the position which he occupied until he became emeritus professor in 1931. He died unmarried, June 15, 1936.

Although he always regarded teaching as his first obligation, he served as secretary of the University faculty, dean of the College of Arts and Sciences, and faculty member on the Board of Trustees. He also participated actively in community affairs, such as the Co-operative Society, the Town and Gown Club, the Community Chest, the Hospital Association, the Board of Education, the Chamber of Commerce, and Cornell University Library Association.

Professor Hull published little, but not withstanding this fact, he achieved high distinction as a scholar. His edition of *The Economic Writings of Sir William Petty*, published when he was thirty-four years of age, was at once pronounced by competent critics a masterpiece of its kind.



*Charles W. Bull*

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## RELATIVE PRICES AND AGGREGATE SPENDING IN THE ANALYSIS OF DEVALUATION

By FRITZ MACHLUP\*

Two approaches to the question of the effects of devaluation have been presented as alternatives, and one of them has been treated as inferior, if not absolutely inappropriate.<sup>1</sup> The purpose of this article is to examine the supposedly superior approach and to give a comparative evaluation of both.

The problem is how to analyze the probable effectiveness of a devaluation undertaken to remove or reduce an existing excess demand for foreign exchange without the use of direct controls, when money incomes have been stable, and when no autonomous capital movements take place either before or after the devaluation. These restricting conditions serve to present the problem in splendid isolation from certain very realistic conditions—such as the presence of direct controls or of autonomous capital movements—from which abstraction must initially be made in a clean analysis.

We are not concerned here with the question whether devaluation is the "most appropriate" policy under given circumstances, or under what circumstances devaluation would be "more appropriate" than other policies. Our question is merely this: what is the best way of finding out whether devaluation will reduce the trade deficit, or what per cent of devaluation would eliminate a given trade deficit, or what size of a deficit would be eliminated by a given per cent of devaluation?

I refer to the two ways of analyzing the problem as the *relative-prices* approach and the *aggregate-spending* approach. Alexander called them the "elasticities approach" and the "income-absorption ap-

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<sup>1</sup>S. S. Alexander, "Effects of a Devaluation on a Trade Balance," *Internat. Mon. Fund Staff Papers*, Apr. 1952, II, 263-78.

proach."<sup>2</sup> There are other possible names: the "supply-and-demand approach" and the "income-and-outlay approach"; or, with an allusion to doctrinal history, the "Marshallian approach" and the "Keynesian approach" to the problem.

### I. *Deficiencies of the Relative-Prices Approach*

The most widely used version of the relative-prices approach works with supply and demand curves for foreign exchange. The positions and shapes of these curves, and thus their elasticities at various points, are "deduced" from the supply and demand for exports and from the supply and demand for imports; and all these, in turn, are "deduced" from the supply conditions and demand conditions in all foreign and domestic markets, because every supply in foreign trade is an excess supply over domestic demand, and every demand in foreign trade is an excess demand over domestic supply.

Now the question has been raised whether one can take the shape of any of these curves as given; whether one can regard their elasticities as predetermined and use them in the solution of foreign-exchange problems. I had thought it useful to assume so,<sup>3</sup> and so had many others.

#### *Given and Unchanged Cost Conditions*

We said, for example, that the supply of exports would depend in part on "cost-conditions"—as if cost conditions were something definite. But is this defensible? The cost of *one* product can be shown in a curve, other things being given and unchanged; but if other things change, the cost curve no longer stays put; if the production of many goods is to change at the same time, we cannot easily know what the cost curve for any one of them will do.<sup>4</sup>

In the production of a certain good, productive resources are needed; for the production of more of that good, more resources are needed. If not much is happening elsewhere, one may foretell at what prices additional resources may be available, and one may, on the basis of technological knowledge, foretell what the cost of additional output will be. But for the prices of additional resources it will make a great difference whether at the same time these resources are being released by other industries or are being demanded by them in increased amounts.

<sup>2</sup> Alexander obviously means price elasticities by the first name, since income elasticity of demand plays a main part in the second approach.

<sup>3</sup> Fritz Machlup, "The Theory of Foreign Exchange," *Economica*, Nov. 1939, VI, 375-97, and Feb. 1940, VII, 23-49. Reprinted in *Readings in the Theory of International Trade* (Philadelphia, 1949), pp. 104-158.

<sup>4</sup> For an excellent exposition of the general problem of cost and supply curves, see Joan Robinson, "Rising Supply Price," *Economica*, Feb. 1941, VIII, 1-8. Reprinted in G. J. Stigler and K. E. Boulding, ed., *Readings in Price Theory* (Chicago, 1952), pp. 233-41.

It is possible that, when the production of a certain product rises, say in order to provide increased exports under the stimulus of a devaluation, the production of other goods may be cut and productive services released to the expanding export industry. But it is also possible that some other industries will increase their output under the stimulus of the same devaluation, and that they also will require more of the same productive factors; and if several industries simultaneously demand these factors, their prices will rise. Where then are our "given" cost conditions?

#### *Given and Unchanged Incomes*

A second argument against the relative-prices approach is the possibility of changes in incomes. The devaluation itself may have income effects: incomes may rise or fall as a result of the devaluation. But the supply and demand curves are drawn, of course, on the basis of a given buying power, a given income. Thus, these curves will not help much if we know that incomes must have changed; we have to know how income has changed and how the new curves look.

Even if we did not allow total money income to change (and we shall return to this point), there is still the possibility of significant changes in income distribution. After all, devaluation will raise the domestic prices of imported goods which may be important in the budgets of certain groups; this would alter the distribution of real income. But if income distribution changes, demand is likely to change and our curves again may shift considerably.

That these arguments make it necessary to qualify and supplement the "simple" elasticity analysis had been clear to many.<sup>5</sup> But what Alexander suggests amounts to a repudiation of the relative-prices approach.<sup>6</sup> His arguments are persuasive and, with our confidence in

<sup>5</sup> The income effects of devaluation have often been discussed. I wrote: "If depreciation through its effects upon the volume of exports or upon the balance or the terms of trade should raise or lower total income, it would probably also raise or lower the demand for imports. These income effects of depreciation may be negligible in the beginning, but in the course of time, as the 'multiplier mechanism' becomes operative, they may become strong enough substantially to weaken or reinforce the price effects of depreciation." "Elasticity Pessimism in International Trade," *Econ. Internaz.*, Feb. 1950, III, 11. See also, J. J. Polak, "Discussion," *Am. Econ. Rev.*, Proceedings, May, 1952, XLII, 180-81; A. C. Harberger, "Currency Depreciation, Income, and the Balance of Trade," *Jour. Pol. Econ.*, Feb. 1950, LVIII, 47-60; T. Balogh and P. P. Streeten, "The Inappropriateness of Simple 'Elasticity' Concepts in the Analysis of International Trade," and "Exchange Rates and National Income," *Bull. Oxford Univ. Inst. Stat.*, Mar. and Apr. 1951, XIII, 65-77, 101-08.

<sup>6</sup> "... the total elasticities appropriate for the analysis of the effects of a devaluation depend on the behavior of the whole economic system, and the statement that the effect of a devaluation depends on the elasticities boils down to the statement that it depends on how the economic system behaves." "... it is suggested that a more fruitful line of

the criticized approach shaken or destroyed, we are eager to examine the novel approach that is offered as a superior substitute.

## II. Alexander's Aggregate-Spending Approach

The new approach to the problem of the effect of devaluation is the aggregate-spending approach, or "income-absorption" approach. While following Alexander's exposition, I shall slightly change the notations.

### *The Fundamental Equation*

The *total income* of a nation can be divided or classified into *consumption* plus *investment* plus *government contribution*<sup>7</sup> plus *exports* minus *imports*; or

$$Y \equiv C + I + G + X - M.$$

This identity is used as the fundamental equation; it can be shortened by merging the first three terms on the right side, that is by calling *C* plus *I* plus *G* by the name "absorption,"<sup>8</sup> or *A*. (The expenditures of households and business and governments are the part of the national income that is being "absorbed.") The two remaining terms of the identity, *X* minus *M*, the difference between exports and imports, constitute the trade balance, signified by *B*. Thus, what we now have is that the *national income* is the sum of the *absorption* by the nation plus its *trade balance*; or

$$Y \equiv A + B.$$

It follows that the trade balance must always be the difference between income and absorption:

$$B \equiv Y - A.$$

The trade balance is negative when the nation absorbs more than its income. The trade balance can improve if income increases while absorption increases less or stays unchanged or falls; or if absorption decreases while income decreases less or stays unchanged.

### *Devaluation Effects on Income and Spending*

The question we now have to ask is to what extent devaluation can

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approach can be based on a concentration on the relationships of real expenditure to real income and on the relationships of both of these to the price levels, rather than on the more traditional supply and demand analysis." Alexander, *op. cit.*, pp. 264, 263.

Balogh and Streeten also rejected the "elasticities approach": "... we shall contend that this approach to the problem is erroneous" (*op. cit.*, p. 65). But they did not suggest an alternative one: "It is regrettable but inevitable that no single new method of analysis can be put into the place of the old approach" (*ibid.*, p. 66).

<sup>7</sup> This term was not included in Alexander's exposition.

<sup>8</sup> This term was first used by K. E. Boulding, *Economic Analysis* (New York, rev. ed., 1948), pp. 402-3.

affect  $B$ , that is, the difference between income and absorption. Alexander breaks this down into three questions:<sup>9</sup> (1) How does devaluation affect income ( $Y$ )? (2) How does a change in income ( $\Delta Y$ ) affect absorption ( $A$ )? (3) How does devaluation directly (that is, not via income) affect absorption ( $A$ )?

The change of the trade balance is the change in income minus the change in absorption; or

$$\Delta B \equiv \Delta Y - \Delta A.$$

The effect of devaluation upon income will show in  $\Delta Y$ ; the effect of the change in income upon absorption, and the direct effect of devaluation upon absorption, will both show in  $\Delta A$ . In other words, there will be an income-induced change in  $A$  and a nonincome-induced, or directly effected, change in  $A$ . The direct effect of the devaluation upon absorption can be expressed by  $\partial A$ . The income-induced change in absorption can be expressed by  $\alpha \Delta Y$ , where  $\alpha$  is the "marginal propensity to absorb income" (which of course will be the sum of the marginal propensities to consume, to invest and to spend public funds).

The new symbols help to merge the devaluation effect upon income,  $\Delta Y$ , and the income effect upon absorption  $\alpha \Delta Y$ , into a single expression  $(1 - \alpha) \Delta Y$ , which stands for the nonabsorbed change in income. Thus,

$$\Delta B = (1 - \alpha) \Delta Y - \partial A.$$

Assume, for a moment, that income will increase as a result of devaluation. This devaluation effect upon income and the consequent income effect upon absorption will improve the trade balance only if  $\alpha$  is smaller than unity. But, while the marginal propensity to consume is usually smaller than unity,  $\alpha$ , the combined marginal propensity to consume, invest, and spend publicly, may well be greater than unity. If so,  $(1 - \alpha) \Delta Y$ , the nonabsorbed change in income, will be negative and the trade balance will deteriorate, rather than improve, on this score. Only the direct effect on absorption can then still help matters.

Alexander states that the analysis on the basis of his model will hold in real terms as well as in money terms; he then proceeds in the belief that he talks about real income, real absorption, and real trade balance throughout.

Following the clue from the last equation, Alexander divides the further discussion into two parts: the effects upon and via incomes—

<sup>9</sup> *Op. cit.*, p. 266.

$(1 - \alpha) \Delta Y$ —and the direct effects upon absorption— $\Delta A$ . He recognizes the following effects:

Effects upon and via Income [(1- $\alpha$ ) $\Delta Y$ ]	Direct Effects on Absorption [ $\Delta A$ ]
Idle-resources effect Terms-of-trade effect	Cash-balance effect Income-redistribution effect Money-illusion effect Three other direct absorption effects

### *The Idle-Resources Effect*

If the devaluing country has idle resources, their employment can be increased by additional consumption, investment, government expenditures, or exports. Since  $C$  and  $I$  (and tacitly,  $G$ ) are assumed by Alexander to be functions of income, additional exports are the strategic factor; and additional exports are the very thing that one should expect to result from devaluation.

The increased value of foreign moneys in terms of domestic money stimulates the production of export goods; if idle resources are available, employment will increase in the export industries; as the recipients of the income increment spend it for consumption, further employment may be created in the consumers-goods industries. This process is the familiar operation of the "foreign trade multiplier." But while the multiplier, in the customary exposition, includes only induced consumption, Alexander's employment effect comprises induced spending of all kinds.

Alexander inclines to the belief that  $\alpha$  is greater than unity, that  $(1 - \alpha)$  therefore is negative, and consequently that the trade balance will become worse as a result of the income increase due to the idle-resources effect of the devaluation. If improving the trade balance is the policy objective, it would follow that the devaluing country must hope either that there will be no increase in its income and employment, or that the propensity to absorb income is less than unity—or at least that the other effects of devaluation will be in the right direction and stronger than the idle-resources effect.

### *The Terms-of-Trade Effect*

Alexander joins the majority of economists in the belief that devaluation will deteriorate the terms of trade of the devaluing country. He thinks that this is so because "a country's exports are usually more specialized than its imports."<sup>10</sup>

<sup>10</sup> *Ibid.*, p. 268.

If devaluation affects the terms of trade, the change in the terms of trade will affect national income and absorption. Alexander divides the terms-of-trade effects of devaluation on the balance of trade into an initial effect through price changes and a secondary effect through income-induced changes in absorption. He holds that "the normal result of a devaluation will be such deterioration of the terms of trade of the devaluing country as to" cause an initial deterioration of its balance of payments equal to "the reduction of the country's real income associated with the deterioration of the terms of trade."<sup>11</sup> In other words, the initial effect will normally be an equal and simultaneous reduction (deterioration) of the trade balance and of real national income. The secondary terms-of-trade effects of devaluation upon the balance of trade—the income-induced changes in spending—will depend on the marginal propensity to absorb income. The income-induced changes in absorption may either reinforce or attenuate the initial terms-of-trade effect on the balance of trade, or may turn it in the opposite direction.

Alexander, having concluded that the *initial* terms-of-trade effects upon trade balance and upon real income are (normally) equal in direction and in amount—he uses one symbol,  $t$ , for both—believes that he can find the ultimate terms-of-trade effect upon the trade balance by multiplying the initial effect by the marginal propensity not-to-absorb. Hence, the ultimate terms-of-trade effect upon the balance of trade would be  $(1 - \alpha)t$ , and thus could be positive (*i.e.*, improving the balance) only if (since  $t$  is assumed to be negative)  $1 - \alpha$  is negative, that is, if  $\alpha$  is greater than unity.

#### *The Income Effects Combined*

Alexander combines the two "income effects of devaluation" which he recognizes—the idle-resources effect and the terms-of-trade effect—in one expression equivalent to  $(1 - \alpha) \Delta Y$ . This presupposes that a change in the terms of trade resulting from devaluation will affect the balance of trade only through the change in real income, and to an extent commensurate with it.

Either or both of the income effects of devaluation may be zero. The idle-resources effect—"presumably positive," according to Alexander—can be positive only if there are unemployed resources and if their employment is not obstructed by bottlenecks (lack of complementary factors or lack of finance). The terms-of-trade effect may be zero if devaluation does not change the terms of trade, or if the "initial" effects on the trade balance as well as the "secondary" effects

<sup>11</sup> *Ibid.*, p. 269.

through income-induced changes in spending are checked or counteracted by monetary policy.

### *Direct Effects on Absorption*

Effects of devaluation upon the balance of trade which are not associated with changes in income but only with changes in the absorption of a given income, are called "direct effects on absorption." If there is no change in income, the trade balance can be increased only through a reduction in the domestic absorption of income,  $C$ ,  $I$ , or  $G$ , and an equal increase in exports or reduction in imports. Such a switch from  $A$  to  $B$ , from domestic absorption to foreign-trade balance, can possibly be accomplished without transfer of productive resources or changes in production if the reduction in domestic absorption relates to imported goods or to exportable goods. Otherwise it will require a shift of production from goods and services hitherto used for  $C$ ,  $I$ , or  $G$  to goods and services for  $X$ , or to goods and services substituted for  $M$ . This will involve not only adaptations in production plans but ordinarily also a transfer of productive resources between plants, firms, industries, and locations. The question is, what mechanisms are set in motion by devaluation to induce these adaptations and transfers, and what obstacles have to be overcome in the process?

A reduction in consumption and investment can, in general, lead to a reduction in employment and income just as easily as to an increase in the trade balance.<sup>12</sup> Whether the productive resources released from domestic  $C$  and  $I$  industries will remain idle or will be transferred to industries producing  $X$  and substitutes for  $M$ , will depend chiefly on how the economy responds to price incentives; this need not involve price reductions, because the release of the resources will have been preceded by an increase in prices of export goods and of substitutes for import goods. Moreover, the reduction in spending for  $C$  and  $I$  will have been offset by an increase in receipts for  $X$ . But the chief object in Alexander's discussion of the "direct effects on absorption" is to examine whether and how devaluation may bring about the reduction in domestic spending.

### *The Cash-Balance Effect*

Devaluation raises the domestic prices of imports and of exports; and it will tend to raise the prices of import substitutes, of potential exports, and of intermediate goods required for their production. Thus, unless the monetary authorities restrict credit in order to force price reductions in other sectors of the economy, the price level will be somewhat increased as a result of the devaluation.

<sup>12</sup> *Ibid.*, p. 272.

If the monetary authorities do not create more money than may be needed to purchase the foreign exchange forthcoming as a result of positive devaluation effects on the trade balance, the elevated price level will imply a reduction in the real value of total cash balances. Households and firms will attempt to build up their cash balances to the relative size they have found appropriate. They will try to do this by buying less and by selling assets and securities (debts).

Buying less in order to accumulate cash balances implies "a reduction in their real expenditures relative to their real incomes,"<sup>13</sup> that is, a foregoing of consumption and investment. Selling assets and debts securities will depress their prices or, which is the same thing, increase interest rates. The offer of assets and debt securities at reduced prices would attract foreign buyers—which would greatly help matters—but we have excluded any autonomous capital movements from our analysis. With foreigners ruled out as buyers—because we wish to abstract from capital inflows in order to concentrate on the trade balance—and with banks ruled out as buyers—because we have excluded additional credit creation—there are only nonbank residents left as buyers, and they will overcome their liquidity preference only if the prices of securities are so attractive that it pays them to defer real investment, reduce consumption, and part with liquidity. The increased interest rates will have the effect of cutting down investment expenditures of business and consumption expenditures of households.

#### *The Income-Redistribution Effect*

The lift in the price level that is associated with devaluation may also reduce aggregate spending from a given income by redistributing it from groups with higher to groups with lower marginal propensities to spend. Alexander mentions three such shifts of real income: from fixed-income recipients "to the rest of the economy,"<sup>14</sup> from wage recipients to profit recipients, and from taxpayers to government.

A loss of real income which the recipients of fixed income suffer through increased prices will result in a reduction of aggregate absorption if the corresponding gain in real income accrues largely to people richer and thriftier than rentiers, fixed-salary workers, and pensioners—which is not unlikely.

A shift of real income away from wage earners will occur when wage goods are among those whose prices are raised through devaluation—which is rather common. If the shift of real income is towards profit recipients, their investment incentives may be increased by more than

<sup>13</sup> *Ibid.*, p. 271.

<sup>14</sup> *Ibid.*, p. 273.

the consumption demand of the real-income losers is reduced. And if investment outlays increase accordingly, total absorption of income may be increased rather than reduced.

A shift from taxpayers to government would most effectively cut down aggregate spending where government expenditures are not dependent on tax revenues but are fixed in an inflexible budget not to be stepped up with a larger flow of tax receipts. Income-tax receipts would be increased where an income redistribution takes place in favor of richer people and tax rates are progressive. And income tax receipts would also be increased where total money income is allowed to increase in consequence of devaluation.<sup>15</sup>

### *The Money-Illusion Effect*

This is what Alexander has to say on the money-illusion effect:

The money illusion may contribute a favorable effect to a devaluation if it actually leads people to pay more attention to money prices than to money incomes. If at higher prices people choose to buy and consume less *even though their money income has increased in proportion*, over and above what can be attributed to the cash balance effect, the result on the balance of payments will be favorable. But *rising money incomes and rising prices may actually operate in the opposite manner*; for example, annual savings may be calculated in money terms and may fail to rise in proportion to money incomes and prices.<sup>16</sup>

It should be noted that Alexander speaks here, in the italicized clauses, not of higher prices of foreign-trade-connected goods, but of rising prices and of money incomes rising in proportion.

### *Other Direct Absorption Effects*

Three other direct effects of devaluation upon absorption, and thus upon the trade balance, are mentioned by Alexander. One of them, which we may call the "price-expectations effect," may unfavorably affect the trade balance by increasing absorption: people expecting prices to rise following the devaluation may rush out to increase their inventories.

The other two are favorable influences on the trade balance. What may be called the "high-cost-of-investment effect" consists in the discouragement which increased cost of imported investment goods may cause to investors. Investment that requires foreign equipment, to be

<sup>15</sup> On the other hand, the revenues from specific import duties may be reduced rather than increased, and in many countries import duties are a significant portion of the budget.

<sup>16</sup> *Loc. cit.* Emphasis supplied.

imported at increased domestic prices, may be less attractive than it was before the devaluation and may be cut out altogether.<sup>17</sup>

The third effect generalizes the principle involved in the second effect to all kinds of imports: when certain goods, previously imported, become very expensive, some of the domestic buyers may give up buying these goods and may buy nothing instead. Alexander mentions this merely as a "theoretical possibility."

### III. *Some Consequences of Neglecting Relative Prices*

My exposition of Alexander's analysis is, I hope, fair and accurate. In the first part of my critique I shall—without questioning the validity of the framework of the analysis or the merits of the procedure employed—point to some omissions and errors of reasoning which I believe can be attributed to Alexander's concentration on aggregate magnitudes and his neglect of relative prices. Although Alexander does not explicitly state that his enumeration of "income effects" and "direct absorption effects" is exhaustive, an impression is conveyed that, if not all, at least the more important ones have been covered. This, however, is not the case; we can find omissions in both categories.

#### *The Resource-Reallocation Effect*

There are three ways in which an increase in real national income can be achieved: through fuller employment of the available productive resources; through their better utilization and more economic allocation; and through more favorable terms of trade. Only two of these possibilities are recognized in Alexander's analysis: changes in the volume of employment and in the terms of trade. Transfer of resources to different uses does play a considerable role in his analysis, but only in connection with the direct absorption effects of devaluation. That such transfer may change real national income is not mentioned and has evidently been overlooked.

In the long run, greater economy and efficiency in the use of resources has been the most important factor in the increase in the living standards of the nations. In the short run, changes in the volume of employment and in the terms of trade may overshadow the effects of changed resources-use upon real income; but there is no presumption for one or the other to be more important. All three kinds of changes may be effected by devaluation (and each of them may be positive or negative).

<sup>17</sup> This seems to have been of considerable practical importance in several countries: Investment in imported labor-saving machinery, very profitable at predevaluation exchange rates, turned out to be too expensive relative to domestic labor once the exchange rates were corrected.

The "resource-reallocation effect" of devaluation may be especially significant when the "idle-resources effect" is negligible or zero; total employment may remain practically unchanged while the output produced may increase through a more economic or more efficient use of the resources employed. But it is also possible that both effects operate at the same time; more or fewer resources may be employed in a more or less economic way.

There are problems involved in measuring increases in real income when there are changes in the composition of output. Where the reallocation of resources implies a shift to "more valuable" products, with reductions in the output of "less valuable" products, the gain in real income can be measured only by means of a welfare index (based on market prices combined with other criteria). Even so, the principle that a reallocation of resources may increase the value of real output cannot reasonably be questioned. And where trade and production have been conducted on the basis of "unrealistic" exchange rates (overvaluing the currency) it is quite plausible that devaluation may effect a more economic use of resources with a consequent increase in real income.<sup>18</sup>

#### *Substitution Effects*

The resource-reallocation effect has been named here as another income effect of devaluation, although in Alexander's analysis resource reallocation is treated only in connection with direct absorption effects. But all of the direct absorption effects discussed by Alexander have to do with aggregate spending, with changes in the outlay of money; if they "work" to reduce total absorption they do so owing to the failure of some income recipients to spend all of their receipts or to make all the money expenditures they would otherwise have made.<sup>19</sup> Perhaps the most important absorption effects are thereby disregarded: the effects of shifts in relative prices and the effects of price increases which reduce real absorption even if absorption in money terms should be unchanged or slightly increased. This reduction in real absorption

<sup>18</sup> Besides the more economic resource allocation achieved through changes in relative prices, there may be two incidental resource-economizing effects of devaluation in systems operating with direct controls: one is the saving of administrative cost by government and by business when devaluation permits some controls to be removed and others to be improved; the second is the improved efficiency under the pressure of revived competition as the industry quota arrangements implied in the bureaucratic allocation of foreign exchange and imported materials are dropped when devaluation reinstates prices in the function of resource allocation.

<sup>19</sup> For example, in connection with the third of the three "miscellaneous direct absorption effects" Alexander says that "when the domestic prices [of imported goods] rise, the domestic purchasers cut their expenditures on these goods but save or hoard the difference, rather than shift the expenditures to other goods." *Op. cit.*, p. 274.

may be additional to that induced by a reduction in real income, although the increased prices may reflect such a real-income reduction.

Assume, for example (in order to isolate the outcome examined here from any terms-of-trade effects), that devaluation leaves the terms of trade unchanged, lifting the domestic prices of exports and of imports by the same percentage. As imported goods are now relatively higher in price than domestic goods, substitution in consumers' plans seems inevitable; and as exportable goods are now relatively higher in price than domestic goods, substitution in producers' plans seems inevitable; transfers of productive resources will ensue. And the increased demand for import-substitutes together with the reduced supply of domestic goods from the production of which resources have been diverted cannot but cause relative price movements which are apt to reduce the real value of aggregate absorption even if total money expenditures should be somewhat higher than before.

This reduction in real absorption will be additional to that induced by a reduction in real income if some among the substitutions induced take the form of shifts between consumption and asset holding (or indebtedness) or between investment and liquidity. Let us not forget that an import surplus implies an increase in indebtedness or decrease in the holdings of securities or other liquid assets on the part of some who absorb (consume or invest). Changes in relative prices may affect the willingness of "absorbers" to run up their debts or run down their liquid asset holdings. Substitution effects may significantly influence the absorption of real income and the physical volumes of imports and exports; these are price-induced changes in absorption, not predicated on any given "propensity to absorb" or on any general price level increase.

In all four kinds of direct effects on absorption discussed by Alexander, price movements play some role. But Alexander relies little on changes in *relative* prices to bring about the required adaptations and transfers.<sup>20</sup> Most of the time he looks to increased or rising price levels to do the trick. No wonder that he is disappointed in the performance.

#### *The Terms-of-Trade Effect Amended*

The neglect of the substitution effects impairs some of Alexander's analysis of the terms-of-trade effect of devaluation. It can be shown—although only in a lengthier exposition than we can here afford—that Alexander is in error when he holds that the initial effect—before any income-induced changes in absorption take place—will "normally" be an equal and simultaneous reduction (deterioration) of the trade

<sup>20</sup> Except in connection with the high-cost-of-investment effect.

balance and of real national income. To be sure, these two reductions can be made equal by definition, but it would be a rather useless definition; otherwise there is nothing that would cause the initial changes in trade balance and real income to be normally equal in amount or even in direction.

The source of Alexander's error in reasoning about the terms-of-trade effect lies in the conceptual decision to treat all effects of devaluation either as effects upon and via income or as direct effects upon absorption. Since a change in the terms of trade will affect income, Alexander at once puts the terms-of-trade effects under the first heading, and fails to notice that a change in the terms of trade will also affect absorption directly, through relative-price effects.

A change in the terms of trade may be viewed as a change in the ratio of an index of export prices to an index of import prices. The effects of relative price changes are customarily (following Hicks) divided into simultaneous income effects and substitution effects. By assuming that a change in the terms of trade will "initially" affect only income but not absorption, Alexander loses sight of the substitution effect. Absorption will actually be affected through both the substitution effects and the income effects of the change in the terms of trade. Thus, Alexander's conclusion that the ultimate terms-of-trade effect upon the balance of trade would be  $(1 - \alpha)t$ , or equal to the income effect times the marginal propensity not-to-absorb, is wrong.<sup>21</sup>

#### IV. Reasoning from Definitional Equations

The argument underlying the aggregate-spending approach has been developed from a "fundamental equation" which represents mere definitions. Such equations usually serve a useful purpose in aiding the organization of the analysis. But they may easily tempt an analyst into "implicit theorizing," illegitimately deducing causal relationships, and overlooking the shifting meanings of terms in different contexts. These temptations have not been successfully resisted in this instance.

##### *Income and Output, Money Terms and Real Terms*

As Alexander interprets the fundamental equation (or identity), "real income . . . is equal to the output of goods and services," and the relationships expressed "hold, of course, both in real and in money terms."<sup>22</sup> While this makes good sense for a closed system, it does not

<sup>21</sup> A clear exposition of the rather complicated relationships involved requires considerably more space, particularly because some fundamental questions concerning the use of terms-of-trade analysis ought to be explored at the same time. I reserve these tasks for a separate article.

<sup>22</sup> *Op. cit.*, p. 266.

for an open one, especially when the balance of trade plays a major role.

Even in a closed system there may be difficulties—due to problems of depreciation and depletion—in equating real income and output; but these difficulties can be defined away. In an open system where some output is produced for export, and imports contribute to consumption and domestic investment, output produced is not the same as real income. In order to relate output and income one would have to take account of changes in foreign assets and debts, as well as of unilateral transfers—items for which no provision is made in the equation. It is possible for employment, production, consumption, domestic investment, exports and imports, all to remain absolutely unchanged in physical terms, and for real national income nevertheless to be changed by some of the exports being sold at increased or reduced prices or given away free, or by some of the imports being bought at increased or reduced prices or received as gifts.

The conceptual difficulties implied in the possible deviations between changes in national product and national income<sup>23</sup> may have a significant bearing on the analysis of the effects of devaluation, especially in connection with changes in the terms of trade. What, for example, is the relevance of a "given" marginal propensity to absorb, when output and employment are affected one way, but income (with changes in foreign debts or assets) in another way? Will changes in absorption be induced by changes in employment and production or rather by changes in income that are partly due to changes in the foreign exchange reserves?

The supposed equivalence between relationships in money and in real terms is sometimes troublesome, occasionally even meaningless. Alexander intends to "deal only with real quantities, not with money values."<sup>24</sup> But what is a "real" trade balance? When a country has to pay increased prices for its imports and buys a slightly reduced physical quantity of imports, what is the meaning of an improved "real" trade balance if at the same time the balance in money terms has deteriorated? Or, should, perhaps, a trade balance in terms of money be translated into one in real terms by deflating the contracted foreign claims or debts by a price index? To treat exports and imports as "real quantities" makes sense; but can the difference between the two be meaningfully treated as a real (physical) quantity?<sup>25</sup> Can what is

<sup>23</sup> More correctly, possible deviations between changes in the total production of goods and services and in the total amount of income earned by the members of the economy. We are not concerned here with the difference between national income and disposable income.

<sup>24</sup> *Op. cit.*, p. 266.

<sup>25</sup> While horses and apples cannot be added, apples can be added to horses, but never subtracted from horses.

essentially an increase or decrease in foreign claims or debts be regarded as anything but a money value, however deflated?

### *Trade Balance in Foreign or in Domestic Money*

But this is not all. Even if the concept of a trade balance in real terms is given up, the fact remains that, as a result of devaluation, an import surplus may increase in terms of domestic money and decrease in terms of foreign money. Indeed, this is a rather probable outcome.<sup>28</sup> But the  $\Delta B$  in the equation  $\Delta B \equiv \Delta Y - \Delta A$  is expressed either as a domestic money value or as the "real" equivalent of a domestic money value. This  $\Delta B$  may be negative, so that a negative  $B$  may become even more negative, while the trade balance in terms of foreign money actually improves.

If one realizes that the whole purpose of the analysis is to find out what will happen to the trade balance of a country resorting to devaluation because of a shortage of foreign currency, one may be quite disappointed about the wrong result obtained. But, serious though it may sound, the damage is easily repaired by dividing  $B + \Delta B$  by the increased exchange rate of the foreign currency: the deterioration of the balance in terms of domestic money may then show an improvement in terms of foreign money. It remains true, however, that  $\Delta Y - \Delta A$  will not directly give the right answer.

### *Causal versus Ex Post Relations*

The fundamental equation in the analysis, in the form  $Y \equiv C + I + G + X - M$ , is helpful in organizing an examination of the relationship between the components of aggregate spending and the foreign balance, but misleading if it is deemed to show causal (*ex ante*) rather than classificatory (*ex post*) relationships. For example, an increase in consumption expenditures may result in an increase in  $Y$  (if employment rises) or a decrease in  $I$  (if inventory is depleted) or a decrease in  $X$  (if exportable goods are domestically used) or an increase in  $M$  (if imports are purchased) or no real change at all (if prices of consumption goods rise); and there are many other more indirect possibilities and combinations—the study of which may be underemphasized by overconfident reliance on the "insight" afforded by the equation.

As a matter of fact, the equation gives very little insight into causal relationships. The merging of planned investment and unintentional inventory accumulation into one term,  $I$ , which in turn is part of  $A$ , is an example of an actually misleading "clue" suggested by the equation. Reduced consumption and reduced planned investment may some-

<sup>28</sup> See Machlup, "Theory of Foreign Exchange," *op. cit.*, VI, pp. 375 ff. and *Readings*, pp. 104 ff.

times be offset by increased unintended inventory accumulation (due to the unexpected decline in sales). This latter part of  $\Delta A$  is inversely related to  $\Delta Y$  as far as causal relationship is concerned; and, incidentally, the effects of devaluation upon planned inventory holdings may causally be more important than any positive relation between income and inventory investment.

The difference between causal and mere *ex post* relationships can perhaps be made clearer by asking for the meaning of the equation  $\Delta Y \equiv \Delta A + \Delta B$ . Obviously, if any two of these terms are given, the third can be calculated. But this is not to say that  $\Delta B$  "depends" on  $\Delta Y - \Delta A$  in any causal sense. To say this has no more merit than to say that  $\Delta A$  "depends" on  $\Delta Y - \Delta B$ , or that  $\Delta Y$  "depends" on  $\Delta A + \Delta B$ .

Yet the "income-absorption" approach does rest on such "dependence" if it proposes that an investigation of the effect of devaluation upon the trade balance should proceed by analyzing the "basic questions" how devaluation affects both income and absorption.<sup>27</sup> With the same justification—or lack of justification—one might investigate the effect of devaluation upon national income by analyzing the "basic questions" how devaluation affects both absorption and the trade balance. And an analogous procedure might be proposed for an investigation of the effects of devaluation upon consumption and investment. Such an analytical merry-go-round is entirely "in character" whenever definitional equations furnish the sole basis for inquiry into presumably causal relationships.

#### *Reversing the Direction of Causation*

The income-absorption approach of analyzing the effect of devaluation upon  $B$  assumes that causation goes this way: devaluation affects  $Y$ ;  $Y$  affects  $A$ ; devaluation affects  $A$  directly; the net changes of  $Y$  and  $A$  determine the change of  $B$ .

At one point, however, the question of a reversible process is openly raised: if devaluation affects  $A$  directly, cannot  $A$  affect  $Y$ ? No doubt, it can. If absorption is reduced as a direct result of devaluation, this may cause unemployment, partly or fully offsetting a positive idle-resources effect (due to increased activity in export industries) or even resulting in a net decline in employment and output.

In the event and to the extent that the directly effected reduction in absorption causes a net decline in employment (instead of the desired transfer of resources), a sequence of secondary nonspending will cut down consumption and investment even further, depending on the propensity to absorb; and if the transfer of resources should

<sup>27</sup> Alexander, *op. cit.*, p. 266.

still fail to take place, at least the purchase of imports will be reduced, with a definitely positive effect on the trade balance. Needless to say, no government would want to have the improvement of the trade balance take this form, but it is only fair to mention that it can happen. Analysts, however, may exclude this sequence of events by assuming that the government, pursuing a policy of full or high-level employment, will succeed in preventing a decline in employment.

#### *V. Implicit Shifts of Emphasis*

The income-absorption approach and the fundamental truism on which it rests may be helpful for the purpose of impressing the responsible leaders of the state as well as their economic advisers with the need "to recognize that, if the foreign balance is to be improved, the community as a whole must reduce its absorption of goods and services relative to its income."<sup>28</sup> At the same time the approach may be misleading in that it conceals the dependence of  $B$  (or of  $Y - A$ ) on some key facts which can only be neglected at the risk of perilously obfuscating the policy problem.

#### *The Role of Gold and Exchange Reserves*

If no loans, investments, or repayments are received from abroad, there cannot be any negative  $B$  unless someone in the country is willing to give up foreign balances or other foreign assets. Ordinarily, the monetary authorities are the only willing sellers of foreign balances (or gold). If they do not sell, or have none to sell, the import surplus no longer exists; the real problem is not how to improve the trade balance, but whether to avoid its "automatic" improvement by the operation of "cruel" market forces—and how to make the unavoidable correction of the trade balance least painful or least harmful to the economy.

In a free market for foreign exchange—with no rate-pegging, rate-fixing, or rationing controls—there would surely evolve exchange rates at which all excess demand for imports is eliminated. One may ask *how*—by what forces and strains and stresses—this depreciation of the currency would succeed in balancing trade, but one cannot doubt that it would. A continuing import surplus, in the absence of autonomous capital imports, presupposes a policy of pegging the exchange rate by selling gold or foreign exchange; it disappears when that policy is discontinued.

#### *The Role of Credit Creation*

If no loans, investments, or repayments are received from abroad,

<sup>28</sup> *Ibid.*, p. 275.

there cannot be any negative  $B$  unless someone in the country is using previously inactive domestic cash balances (which is not likely to go on for very long), or new currency is printed, or the banking system, with the active support of the monetary authorities, engages in a continuing expansion of its loans-and-securities portfolio. If this continuing supply of domestic funds is not explained, one cannot understand how "propensities to absorb" can ever lead to absorption in excess of income.

The continuing supply of new bank credit need not be a continuing net increase in the supply of money. When the monetary authorities sell foreign exchange from their reserves, the domestic money (bank deposits) that is paid for it is canceled. The expansion of the loans-and-securities portfolio of the banks merely recreates the bank deposits canceled by the purchases of foreign balances from official reserves. To put it differently, the current credit expansion finances the current "excess absorption" by consumers and investors as well as the purchase of the foreign exchange that it requires.

Without continuing dishoarding or bank credit expansion, the negative trade balance could not continue. Every day of excess imports would bring a further reduction in the money supply of the people and, inevitably, a decline in absorption. A persistent import surplus, in the absence of autonomous capital imports, presupposes a policy of enabling the banking system to expand credit; it disappears when that policy is discontinued.

The possibility of credit expansion, incidentally, besides being a prerequisite of the maintenance of a negative trade balance and of any deterioration of the trade balance, is also essential in other phases of the operation of Alexander's model. In particular, the working of the idle-resources effect may depend on it. While some increase in employment can usually be financed by hitherto inactive liquid funds, by and large it takes new bank money to do this job.<sup>29</sup>

### *Assumptions about the Supply of Money*

Nothing can be said about the effects of a devaluation unless exact specifications are made regarding the supply of money and credit and

<sup>29</sup> Alexander's attempt to combine his two income effects of devaluation in one expression and to treat them alike suffers not only from the defect that the terms-of-trade effect on the trade balance does not operate solely via income, but also that "finance" is no prerequisite for it. An increase in real income produced by the idle-resources effect must be accompanied by an increase in money income and money flow; on the other hand, a change in real income produced by the terms-of-trade effect may be merely a matter of relative prices and need not be reflected in a change in money income and circulation. This difference, neglected by Alexander, may be significant and has even its policy aspects, because inelasticity of the supply of money may be part of the monetary policy of a country or a built-in feature of its currency system, which would largely inhibit the operation of the idle-resources effect.

the fiscal policy of the government. There seems to be a tendency in the "New Economics" tacitly to regard the supply of money as a dependent variable rather than as a policy variable. In the "very old economics," where models of an unmanaged gold standard still had some applicability, the supply of money could be treated as a dependent variable. But when everybody has views on how the supply of money "ought" to be managed, and when in fact almost every government in the world does manage the supply of money, one may reasonably expect economic analysts to be explicit on this point and to state what happens under the various monetary policies which a government may choose to pursue.

On some occasions Alexander follows this good practice; for example, in discussing the cash-balance effect he first stipulates that "the money supply is inflexible." But in many places he fails to make such stipulations.<sup>30</sup> Indeed, he later returns to the cash-balance effect and calls it transitory because "the money supply may *respond* to the increased demand for cash balances."<sup>31</sup> As economists we should, I submit, make a clear distinction between a "response" explainable in economic terms (such as an increase in quantities of product supplied in consequence of increased effective demand reflected in higher prices and in greater profitability of an increased output) and a "response" explainable in political terms (such as an increase in the quantity of money—associated with increased government expenditures, reduced reserve requirements, increased open-market purchases by the central bank, etc.—either in consequence of political pressures or in anticipation of economic, social and political repercussions considered to be undesirable by the political powers). An economic response will be treated as a dependent variable; a political response should be treated as a policy variable and enumerated among the "special assumptions."

To assume tacitly, as is often done, that the money supply will "respond" to an increased demand for credit (to finance increased wage payments and increased foreign payments) may deprive an analysis, such as that of the effects of devaluation, of much of its meaning. To be sure, where the purpose of devaluation is to stimulate employment through the stimulation of exports, it will be the policy of the authorities to help supply the additional credits that are demanded. Even in this case the analyst should state what policy is assumed. But where the purpose of devaluation is to reduce or remove an excess demand for foreign money (that is, with regard to the external balance, an excess supply of domestic money), a policy of supplying the credit demanded

<sup>30</sup> *Op. cit.*, pp. 270, 273, although he speaks there repeatedly of "rising money incomes and rising prices."

<sup>31</sup> *Ibid.*, p. 274. Emphasis supplied.

to replace the excess demand that was squeezed out by the devaluation is not very consistent—even if it should be politically unavoidable.<sup>32</sup>

## VI. Comparing the Two Approaches

### *Given and Unchanged Parameters*

The reason given for advancing the aggregate-spending approach was that its predecessor the relative-prices approach suffered from incurable deficiencies. The basic trouble of the latter is that it works with price elasticities which presumably are given and knowable, but actually are neither—and are even changed as a result of the very devaluation effects which they are supposed to determine.

The new approach assigns strategic importance to spending propensities. Since these spending propensities are supposed to determine the effects of devaluation upon the trade balance, the impression is created that  $\alpha$ , the marginal propensity to absorb, is both given and knowable. Actually it is neither; indeed, we have every reason to believe that  $\alpha$  is not stable over time and that it may change not only with the mood of the time but also momentarily according to the circumstances of the situation. Although called a "propensity" to absorb income, it contains both intentional and unintentional reactions, including unintended investment or disinvestment in inventories, which sometimes may offset the effects of intentional reactions; it comprises government expenditure (inclusive of public capital expenditure), which is not a function of income but an independent variable that can be administered in a direction opposite to that of changes in income; even in so far as it refers to private actions, it may be significantly influenced by monetary and fiscal policy; and, finally, it may be substantially changed as a result of the very devaluation the effects of which it supposedly determines.

Hence, what was said about the "elasticities approach," namely, that "the statement that the effect of a devaluation depends on the elasticities boils down to the statement that it depends on how the economic system behaves,"<sup>33</sup> may with equal justification be said about the income-absorption relation. For, after all, the devaluation effects upon income and absorption, including the supposedly given "propensities to absorb," depend "on how the economic system behaves"—and, we may add, on how it is made to behave by monetary and fiscal policies.

From the point of view of stability over time it is hard to say which of the parameters are less reliable as indicators of the effects of devalua-

<sup>32</sup> Even in this case a policy of devaluation may be adopted with the idea of taking advantage of lags and of gaining a breathing spell between devaluation and re-inflation.

<sup>33</sup> *Op. cit.*, p. 264.

tion on the trade balance—the price elasticities or the spending propensities. From the point of view of changeability in the very process the outcome of which they help determine, the spending propensities are probably less reliable than the price elasticities. And from the point of view of malleability through public policy, one probably should regard the price elasticities as the tougher factors to deal with, and the spending propensities more subject to the influence of (monetary and fiscal) policy—which means that in the last analysis not given propensities but chosen policies will determine the outcome.

### *Foreign Supply and Demand Conditions*

An explanation of the volume, composition, terms, and balance of trade between nations can hardly be regarded as fully convincing if it takes account of the conditions in only one of the nations concerned instead of considering all parties involved. The relative-prices approach attempts to satisfy this precept by including the supply and demand conditions in foreign markets among the determining factors. In particular the elasticities of foreign demand for the exports and of foreign supply of the imports of the devaluing country are assigned important roles.

The aggregate-spending approach makes no such provision, at least not explicitly. It attempts to deduce the devaluation effects on the trade balance solely from the effects upon national income and absorption in the devaluing country. Since the “resulting” change in the trade balance is necessarily also a change, by the same amount though with opposite sign, in the trade balance of the trading partner—perhaps the rest of the world—one wonders how this change is imposed, so to speak, on the latter, regardless of the magnitudes of their spending propensities, etc. If only the  $\Delta Y$  and  $\Delta A$  of the devaluing country were to determine the outcome, would this not imply that in the other countries changes in income and absorption would be “dictated” by a change in the trade balance, instead of the other way around?

Alexander's analysis is silent on this point. An enterprising builder of aggregative models might set out to construct a two-country model embodying the income-absorption relations based on the spending propensities in both countries affected by the change in foreign exchange rates. But I am not convinced that this would be a worth-while undertaking. It is probably more expedient to assume that “the foreign country” is the rest of the world, that the world-wide income and absorption effects of the devaluation of the one currency are so widely dispersed as to be negligible, and that therefore the elasticities of foreign demand and foreign supply are not sufficiently altered to lose

their determining force on the outcome.<sup>84</sup> But this solution would obviously restore the elasticities approach to at least half its former role in the analysis of devaluation.

### *Price Elasticities in the Spending Approach*

As a matter of fact, this restoration of relative prices and price elasticities to a strategic position in the model is not left to its reconstruction by future renovators; price elasticities have been allowed to continue all along, though inconspicuously, to do their job under the new regime. Supposedly banished under the aggregate-spending approach, they have in fact played important roles in Alexander's analysis.

At some points the elasticities work behind the scene. For example, whether there can be an idle-resources effect of devaluation depends on whether the production and sale of export goods can be expanded, hence, on elasticities of domestic supply and of foreign demand; but this is not explicitly said. At other points the elasticities are clearly visible on the stage. For example, whether a direct devaluation-effect upon absorption will result in a transfer of resources or in unemployment, depends on "how the economy responds to price incentives,"<sup>85</sup> or on the "price differential between the foreign and the domestic markets" and on "the substitutability of domestic goods for imports in consumption, and of resources as between the production of domestic goods and exports."<sup>86</sup> At one point at least, the role of price elasticities is dominant: in the terms-of-trade effect. For it is impossible to come to any conclusion concerning the effects of devaluation upon the terms of trade except on the basis of an examination of the relevant elasticities of supply and demand.

Alexander would probably deny none of this. For, although he regards aggregative analysis as "a more fruitful line of approach," to be adopted in lieu of "the more traditional supply and demand analysis,"<sup>87</sup> he concedes that "supply and demand conditions, in the sense of partial elasticities, may be useful tools in this [income-absorption] analysis."<sup>88</sup> His objection is to discussions in terms of "total elasticities" and he believes that it is "total elasticities for which the conventional formulas alone are valid."<sup>89</sup>

<sup>84</sup> This suggestion was made by Michael Michaely, *Devaluation and Dual Markets under Inflation with Direct Controls*, a doctoral dissertation submitted to Johns Hopkins University, January, 1955.

<sup>85</sup> See above, p. 262.

<sup>86</sup> Alexander, *op. cit.*, pp. 270, 271.

<sup>87</sup> *Ibid.*, p. 263.

<sup>88</sup> *Ibid.*, p. 275.

<sup>89</sup> *Ibid.*, p. 275. These "formulas," as I have understood them, had no other purpose than to suggest qualitative (directional) relationships and quantitative possibilities. Alex-

*Conclusion*

The upshot of all this is that relative prices and elasticities were not really discarded in the analysis of devaluation effects; and that aggregate spending and propensities by themselves cannot possibly do the explanatory job that was assigned to them. Neither of the two "alternative" sets of tools can be spared; both are needed.

Alexander, I am afraid, has confused his readers by presenting the new approach as a substitute for the old. If clearly presented as complementary to the old, and properly amended, the new analysis can be helpful. The new tools fashioned by Alexander cannot replace the old tools of "conventional analysis"; but they can, after some substantial reshaping, increase the usefulness of the latter.

It is the habit of innovators to disparage the old ways of doing things; it is the duty of critics to appreciate the value of the new without depreciating the value of the old. In trying to pare down the exaggerated claims of the innovator they are sometimes overly critical of the new ideas. Lest I have erred in this direction, I should like to end by paying my respect to Alexander's innovating enterprise and to his contribution to the development of international trade theory. A contribution it is, and an especially meritorious one where it gives scope to the roles of both aggregate spending and relative prices.

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ander must have mistaken them for operational devices or for mere *ex post* relationships. If he means "*ex post* elasticities" when he says "total elasticities," he surely misunderstands most of those who have argued in terms of price elasticities. I, for one, have always had *ex ante* elasticities in mind.

## THE THEORY OF FLEXIBLE EXCHANGE RATES

By E. VICTOR MORGAN\*

Until quite recently, it was generally believed that a fall in the exchange value of a currency would improve the balance of trade and worsen the terms of trade of the country concerned. During the inter-war period, when there was a growing consciousness of the difficulties of reducing the domestic price level through a deflationary monetary policy, it became quite fashionable to advocate flexible exchange rates as the best means of maintaining international balance. Underlying this preference was the belief, not only that depreciation would be effective in correcting an adverse balance, but that it would do so at a cost (in the deterioration of the terms of trade) which would be less than the cost of other forms of adjustment.<sup>1</sup>

The growth of interest in exchange flexibility as a policy led naturally to a parallel development of theoretical studies. The regrettable thing, in the opinion of the present writer, is that these studies concentrated on the concepts of elasticity of supply and demand, and largely ignored the fundamental developments in the pure theory of foreign trade, which were going on at the same time.<sup>2</sup> The main object of this paper will be to submit that results based on these elasticities are subject to severe limitations in theory and are misleading in their policy implications. First, however, it is necessary to give a very brief summary of the theory which it is proposed to criticize.<sup>3</sup>

The basic concepts are four elasticities: the home elasticity of demand for imports ( $\epsilon_i$ ), the foreign elasticity of supply of imports ( $\eta_i$ ),

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<sup>1</sup> That is, the idle resources associated with deflation, and the loss of efficiency in the use of resources resulting from direct import restrictions.

<sup>2</sup> Notably the "proportions of the factors" analysis developed by Heckscher, Ohlin and Samuelson: e.g., E. F. Heckscher, "The Effect of Foreign Trade on the Distribution of Income," *Ekonomisk Tidskrift*, 1919, XXI (2), 1-32, trans. in H. S. Ellis and L. A. Metzler, ed., *Readings in the Theory of International Trade* (Philadelphia, 1949), pp. 272-300; B. Ohlin, *Interregional and International Trade* (Cambridge, Mass., 1933); P. A. Samuelson, "International Trade and the Equalisation of Factor Prices," *Econ. Jour.*, June 1948, LVIII, 163-84; *idem*, "International Factor-Price Equalisation Once Again," *Econ. Jour.*, June 1949, LIX, 181-97.

<sup>3</sup> I shall not attempt to give a full list of the many papers in which these matters are discussed, though a number of them will be cited later.

the foreign elasticity of demand for exports ( $\epsilon_e$ ) and the home elasticity of supply of exports ( $\eta_e$ ). Considering only the trade balance, and starting from a balanced position, it has been shown that a small proportionate fall in the exchange rate ( $K$ ) will produce a favorable balance, as a proportion of the original value of exports, of

$$K \cdot \frac{\epsilon_i \epsilon_e (1 + \eta_i + \eta_e) + \eta_i \eta_e (\epsilon_i + \epsilon_e - 1)}{(\epsilon_i + \eta_i)(\epsilon_e + \eta_e)}$$

This must be positive if the sum of the two demand elasticities is greater than unity, and it may be positive, even if the sum of the demand elasticities is less than unity, provided that the supply elasticities are sufficiently small.

The corresponding formula for the deterioration of the terms of trade is

$$K \cdot \left( \frac{\eta_i}{\epsilon_i + \eta_i} \right) - \left( \frac{\epsilon_e}{\epsilon_e + \eta_e} \right) = K \left( \frac{\eta_i \eta_e - \epsilon_i \epsilon_e}{(\eta_i + \epsilon_i)(\eta_e + \epsilon_e)} \right).$$

This will be positive, and the terms of trade will turn against the depreciating country if  $\eta_i \eta_e > \epsilon_i \epsilon_e$ , that is if the product of the supply elasticities exceeds the product of the demand elasticities. Mrs.

Robinson's condition ( $\frac{\eta_e}{\epsilon_i} > \frac{\epsilon_e}{\eta_i}$  in our notation) also follows from this formula.<sup>4</sup>

Each of these elasticities is a derived concept. For example, a fall in the price of an imported good would lead both to an extension of demand and to a contraction of domestic supply (if it were also produced at home). The resulting extension of the demand for imports is made up of the sum of these two changes. In order to set out the relationships concisely let  $D$  = volume of demand,  $S$  = volume of supply, and  $Q$  = volume of imports (all before the price change); let  $h$  = the home country and  $w$  = the rest of the world. Then we have:

$$\begin{aligned}\epsilon_i &= \frac{\epsilon_{hi} \cdot D_{hi} + \eta_{hi} \cdot S_{hi}}{Q_i} \\ \epsilon_e &= \frac{\eta_{we} \cdot S_{we} + \epsilon_{we} \cdot D_{we}}{Q_e} \\ \eta_i &= \frac{\epsilon_{wi} \cdot D_{wi} + \eta_{wi} \cdot S_{wi}}{Q_i} \\ \eta_e &= \frac{\eta_{he} \cdot S_{he} + \epsilon_{he} \cdot D_{he}}{Q_e}\end{aligned}$$

<sup>4</sup> Joan Robinson, *Essays in the Theory of Employment* (London, 1937), p. 21 n.

The condition for a deterioration of the terms of trade as a result of depreciation can then be written:

$$\frac{\epsilon_{wi} \cdot D_{wi} + \eta_{wi} \cdot S_{wi}}{\epsilon_{hi} \cdot D_{hi} + \eta_{hi} \cdot S_{hi}} > \frac{\eta_{we} \cdot S_{we} + \epsilon_{we} \cdot D_{we}}{\eta_{he} \cdot S_{he} + \epsilon_{he} \cdot D_{he}}$$

So far as the quantitative weights are concerned, a high value on one side of this expression is very likely to be associated with a high value on the other. As for the ratios of the respective elasticities, there seems no *a priori* reason for supposing that one need be higher than the other. Apparently depreciation would tend to raise the prices of both imports and exports in relation to those of nontraded goods, but the formula gives us no reason to suppose that one would necessarily rise more than the other.<sup>5</sup>

It thus appears that depreciation may, according to the structure of the economies concerned, cause both the terms of trade and the balance of trade to move either way. Moreover, since the change in the terms of trade depends on the ratios of the elasticities, while that in the balance of trade depends on their absolute magnitudes, there is no necessary relationship between them. So far from moving, of necessity, in opposite directions, it is quite conceivable that both might improve or both deteriorate as a result of depreciation.

Writers who have treated the subject in this way have generally concentrated on the trade balance alone, ignoring invisible items, capital movements, speculation and the hoarding of foreign currencies. In such a model, a tendency for the balance of trade to deteriorate<sup>6</sup> as a result of depreciation implies a situation of unstable equilibrium in the foreign exchange market. No one has assumed that the markets for the goods entering into trade are in unstable equilibrium, so we have the strange phenomenon of a series of apparently stable goods markets producing an unstable currency market, even though the currencies concerned can be used for no other purpose than the purchase of goods. To one who was brought up on the relatively simple notions of traditional economic theory, all this seems more than a little odd. A number of estimates of

<sup>5</sup> Mrs. Robinson gives reasons why depreciation should lead to an adverse movement based largely on the generalization that, "Any one country plays a more dominant rôle in the world supply of those goods which it exports than it plays in the world market for those things which it imports" (*op. cit.*, p. 219). I doubt if this is universally true, but it is certainly true of some countries; and where it is true, our formula suggests that a deterioration in the terms of trade is a likely consequence of depreciation.

<sup>6</sup> In a model which excludes capital movements, there can be no actual deterioration of the balance. The only meaning which can be attached to the term is the creation of the excess of demand for the foreign currency over its supply.

demand elasticities have been published.<sup>7</sup> Most of them have been surprisingly low, some so low as to cast grave doubts on whether depreciation would be effective at all. The result has been something of a revulsion against the policy of flexible exchange rates. If one accepts both the statistical estimates and the theory underlying them, it is hard to avoid the conclusion that, if flexible rates worked at all, they would do so only at the cost of very violent and disturbing fluctuations. This pessimistic conclusion is by no means out of keeping with the general climate of opinion which is prevalent in some quarters. If one cannot correct an adverse balance by reducing domestic prices for fear of unemployment, industrial strife and bloody revolution; and if exchange depreciation is ineffective, then the only way must be through direct controls. Those who, on other grounds, are inclined towards direct controls can thus take some satisfaction in this apparent vindication of their beliefs.

There have been a number of refinements of the theoretical concepts, some of which will be mentioned later, and the statistical estimates have been subjected to devastating criticism by G. H. Orcutt.<sup>8</sup> There has been little attempt, however, to relate the new apparatus to the theory of international values, and it is when viewed in this light that the whole elasticity approach appears, to the present writer, untenable.

The basic weakness lies in the use of the concept of elasticity itself. Its original and proper use is, of course, to describe a simple functional relationship between volume of sales and price. This implies that the commodity must be sufficiently homogeneous to be measured in physical units, and that all other relevant variables (including money income and the prices of all other goods) must be unchanged. These are the familiar assumptions made in the partial equilibrium analysis of supply and demand for a single good, but they are far removed from what actually happens in the course of international trade.

For a full picture of the adjustment process we need to go behind the goods markets to the factor markets, and to follow the sort of analysis made familiar by Ohlin.<sup>9</sup> Suppose that we have two countries, A and B, endowed with factors of production,  $f_1, f_2, \dots, f_n$ . Call the currency of A dollars and that of B sterling. Take the amount of each

<sup>7</sup> E.g., J. H. Adler, "United States Import Demand during the Interwar Period," *Am. Econ. Rev.*, June 1945, XXXV, 418-30; T. C. Chang, "International Comparison of Demand for Imports," *Rev. Econ. Stud.*, 1945-46, XIII (2), 53-67; *idem*, "The British Demand for Imports in the Inter-war Period," *Econ. Jour.*, June 1946, 188-207; *idem*, "A Statistical Note on World Demand for Exports," *Rev. Econ. Stat.*, May 1948, XXX, 106-16.

<sup>8</sup> G. H. Orcutt, "The Measurement of Price Elasticities in International Trade," *Rev. Econ. Stat.*, May 1950, XXXII, 117-32.

<sup>9</sup> *Op. cit.*, esp. pp. 28 seq.

of the factors which can be bought in A for \$1 and find the respective prices of these amounts in country B. Convert these prices into dollars at any given exchange rate, and arrange the factors in ascending order of price. The resulting range might look something like the line  $B_1B'_1$  in the diagram below, where each dot represents a factor of production. The corresponding A prices would be represented by the horizontal line  $AA'$ , since we have taken a dollar's worth of each factor as a unit of measurement. Now suppose the pound to depreciate against the

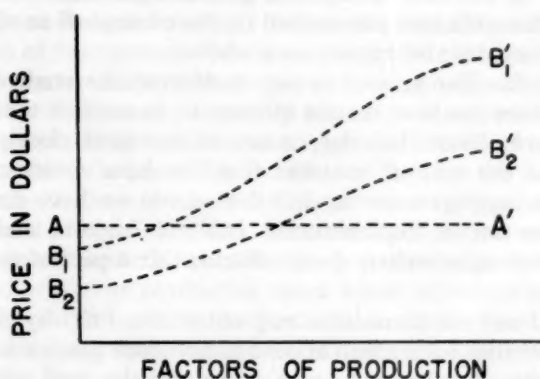


FIGURE 1

dollar. The immediate effect will be to produce an all-round proportionate reduction in the dollar prices of B factors. Instead of  $B_1B'_1$  we shall have a range such as  $B_2B'_2$ . This is the fundamental impact effect of depreciation, a cheapening not of the goods, but of the factors of the depreciating country.

From the cheapening of factors, there naturally follows a cheapening of goods, but the relationship is by no means simple. It will become profitable to export some goods not previously exported, while some which were previously imported will be produced at home. There will also be changes in the relative importance of different goods both in the import and export lists. In the depreciating country there will be an increase in the demand for factors from the export industries and those producing goods competing with imports. Some factors, which are in elastic total supply or which can readily be drawn from other industries, will change little in relative price. Others, for which a strong demand is coupled with an inelastic supply, will rise sharply. Apart from the production functions of the expanding industries and the demand functions for their products, there are at least three influences on the availability of factors: the elasticity of their total supply, their

elasticity of substitution and the elasticity of demand for the products of other industries in which they are used.

Industries using a large proportion of those factors which remain relatively cheap will expand much; others which use a preponderance of factors which are rising sharply in price will have their expansion cut off at an early stage. Similar changes will be going on, in the opposite direction, in the countries which do not depreciate. There is thus a kaleidoscopic movement both among goods and among factors, which will come to an end only with a new general equilibrium of the whole system. All these changes are masked by the concept of an elasticity of demand for imports and exports as a whole.

All this is familiar ground to any undergraduate student of value theory, yet there has been no real attempt to reconcile it with the elasticity approach. Even Haberler, in one of the most closely reasoned expositions of the subject<sup>10</sup> assumes that "we have constructed a sort of average or aggregate curve; in other words we have demand and supply curves for a 'representative bale' of imports and exports" (p. 196). Most other writers do not discuss this aspect of the problem at all.

So far as I can see there is no way out of the difficulty. Any calculation of elasticities for a group of nonhomogeneous goods must measure the relationship between some sort of price index and some sort of index of quantity. No index number can take account of the entry of new goods into trade, or the disappearance of goods formerly traded, and whenever there are changes in the relative importance of different goods, there can be no uniquely right system of weighting. Yet these changes are the very essence of the international adjustment mechanism.

The elasticity of demand for a homogeneous good can be defined in terms of price and quantity which are independent of the statistical techniques used to estimate the elasticity. It is thus possible to conceive of a "true" elasticity and of various more or less close approximations to the truth given by different methods of estimation. With a group of nonhomogeneous goods, however, it is impossible to define elasticity except in terms of the particular methods of estimation which may be used. When we have the type of changes which are bound to occur in the course of international adjustment, none of these methods is uniquely right, and we cannot conceive of a true elasticity. When we use the words elasticity of demand (or supply) for imports (or exports) we quite literally do not know what we are talking about.

We can, of course, make the whole vexatious difficulty fly out of the

<sup>10</sup> G. Haberler, "The Market for Foreign Exchange and the Stability of the Balance of Payments," *Kyklos*, 1949, III (3), 193-218.

window by constructing a simple model in which we assume only two traded goods, one homogeneous import and one homogeneous export. This procedure is of very limited usefulness. It rules out an essential part of the adjustment mechanism of the real world; the construction of the model itself presents some peculiar theoretical problems; and if it is simplified sufficiently to avoid these problems, it can be handled more easily and effectively by an analysis of stability conditions on the lines developed by Hicks than by the use of elasticities.<sup>11</sup> Nevertheless, a brief glance at such a model may be helpful.

First, suppose that we have only two goods, that they are both produced in each of two countries but also traded, and that trade proceeds by means of barter. It would, of course, be impossible for an actual adverse balance to arise. A potentially adverse balance would take the form of an excess of demand for oversupply of the imported good at the ruling market price, and this would tend to raise the price of the import in terms of the export good. This change would set up income, substitution and production effects. The substitution effect would cause a shift in demand in both countries away from the good which had risen in price, while the production effect would stimulate an increase of the supply of that good in both countries. Both these tendencies must be equilibrating, and only the income effect will be ambiguous in direction. The country which has suffered a deterioration in its terms of trade may be expected to consume less of the good in question as a result of its lower real income, while the country whose terms of trade have improved may be expected to consume more. Normally the two influences will tend to cancel one another out, but they would not do so if the good which rose in price were an inferior one for the citizens of one country, but not for those of the other. We thus reach the standard conclusion which applies to a similar situation in a single country; the markets will be stable unless there are very strong and perverse income effects. Since we have excluded money, we cannot have a balance-of-payments problem in this model. But one significant point arises: the correction of a potentially adverse balance involves, of necessity, a deterioration in the terms of trade.

We now introduce into our system what Hicks has called a "shadow money," a money which acts as a *numéraire* but which has none of the other characteristics of money and, in particular, cannot be hoarded. We suppose both our countries to have such a shadow money. There would seem to be nothing to determine the absolute level of prices, and an adjustment might take place either through the exchange market or through a change in goods prices. It can be shown, however, that so long as money can be used only for the immediate purchase of goods,

<sup>11</sup> J. R. Hicks, *Value and Capital* (Oxford, 1939), Pt. 1.

it is impossible to have an unstable exchange market associated with stable commodity markets.

To illustrate this, it is convenient to get rid of the production effect, which is bound to be equilibrating. We therefore assume two countries, A and B, each completely specialized on goods  $\alpha$  and  $\beta$ , respectively, and having currencies  $a$  and  $b$ . Suppose that country A has a tendency to an adverse balance, and that the whole adjustment takes place in the exchange market, the domestic prices of each good in the country producing it being held constant. Constant domestic prices mark the situation in which, according to the elasticities formula, the demand elasticities have to be highest in order to insure stability.

A fall in the  $a$ - $b$  exchange rate will make good  $\beta$  dearer in A, and so reduce the volume of imports and the demand for currency  $b$ . The exchange market will thus be stable unless the demand for the good  $\alpha$  in B is so inelastic that the fall in its price there reduces the supply of currency  $b$  to an extent even greater than the reduction in the demand for it. But if money can be used only for the immediate purchase of goods, a reduction of the supply of  $b$  in the exchange market implies that the citizens of B must be spending correspondingly more of their own currency on their own commodity. Hence if the supply of currency  $b$  is reduced by more than its demand, it follows that the demand for good  $\beta$  from citizens of B increases by more than its demand from citizens of A decreases. The rise in the relative price of good  $\beta$ , brought about by depreciation, has increased the excess demand for it, and the market for the two goods, in the two countries together, is in unstable equilibrium.

The assumption of a shadow money has only a very limited usefulness, yet the transition from a shadow money to a real money is by no means without its difficulties. An essential characteristic of real money is that it can be hoarded, and changes in the propensity to hoard will involve changes in aggregate money income.<sup>12</sup> It thus appears self-contradictory even to set up a model involving a real money which does not take account of fluctuations in income. Fluctuations in domestic income are, of course, bound to occur when an actual change in the trade balance takes place, unless that change is offset by an equal and opposite change in some other component of national expenditure, and this has been realized from the beginning by exponents of the elasticity approach.

<sup>12</sup> Fluctuations in aggregate money income will be associated with fluctuations in real output if they occur at less than "full employment level" and with increases or decreases in inflationary pressure if they occur at above that level. Most writers have assumed idle resources and concentrated on the real fluctuations, but here it seems better to adopt the more general formulation.

The treatment of these fluctuations has given rise to some differences of opinion. Mrs. Robinson<sup>13</sup> regards them as purely a consequence of changes in the foreign balance, and so contends that they can be ignored in considering the direction, though not in considering the extent, of the change which depreciation will cause. Harberger, on the other hand, demonstrates that a change in real output would occur in his model, even if there were no change in the foreign balance, if depreciation led either to a change in investment or to a change in the propensity to hoard.<sup>14</sup> For his model, which assumes constant prices, Harberger finds that the critical value for the sum of the two demand elasticities is not unity, but one plus the sum of the two marginal propensities to import.

The existence of income fluctuations does not necessarily prove fatal to the elasticities approach. Both Harberger and Stuvell<sup>15</sup> have produced elegant and relatively simple mathematical systems which incorporate income fluctuations into the analysis. Both, however, depend upon the existence of marginal propensities to import which are stable enough to be used as parameters. To test the validity of this assumption we need to ask two further questions. Will the price changes resulting from depreciation create changes in the structure of the economy which are likely to alter the relationship between "real income" and imports? And will the changes in imports and exports resulting from a change in income themselves set up further price changes independent of the initial ones? The use of marginal propensities to import in conjunction with elasticities is only justified if both these questions can be answered in the negative, whereas I suggest that the answers to both should be in the affirmative.

We have already suggested that the essence of the adjustment mechanism is a change in relative factor prices leading to a change in the relative importance of different goods in the import and export lists. It would be very surprising if goods which entered into or disappeared from trade responded to changes in income in just the same way as others, and if goods which diminished in importance responded in just the same way as those which increased in importance. Import lists will be different after depreciation from what they were before, and it is only to be expected that they should respond differently to changes in real income.

Moreover, the process of adjustment outlined above involves changes in the relative prices of factors, *i.e.*, a redistribution of income, in both countries, and it is unlikely that the marginal propensity to import

<sup>13</sup> "The Foreign Exchanges," *op. cit.*, pp. 183-209.

<sup>14</sup> A. C. Harberger, "Currency Depreciation, Income and the Balance of Trade," *Jour. Pol. Econ.*, Feb. 1950, LVIII, 47-60.

<sup>15</sup> G. Stuvell, *The Exchange Stability Problem* (Oxford, 1951).

would remain just the same after such a redistribution as it was before.

The changes in factor prices will change both the position and the shape of the cost curves (in each country's own currency) of the domestic industries which compete with imports. This will mean that these industries will react differently from before to changes in demand associated with income changes. Unless we confine ourselves to the assumption of constant domestic prices, we have to consider the secondary price changes (as between imports and domestic goods) which arise from changes in demand which are, themselves, consequences of changes in real income, and these secondary price changes will be different after depreciation from what they would have been before.

Finally, changes in relative factor prices will change the import content of exports. The marginal propensity to import of one country is reflected in the exports of another, and changes in income and imports will therefore be reflected back from one country to another in a different way after depreciation.

I suggest, therefore, that the use of ordinary marginal propensities to import in this context is a simplification which is very likely to be misleading. I do not doubt that a formal solution of these problems could be obtained by constructing a much more elaborate model with a sufficient number of new propensities and elasticities, but the model would probably produce so many equations that an electronic calculator would be needed to solve them. Even then, I suspect, it would only tell us that everything depended on everything else.

So far we have been concerned with two objections to the elasticity approach; that it is impossible to define elasticities which take account of the changes in the composition of import and export lists, which are the essence of the adjustment mechanism; and that it is very difficult, if not impossible, to take adequate account of the changes in income which are bound to be associated with depreciation. A third and final objection is that depreciation is likely to lead to shifts in supply and demand curves, independent of those which may occur through changes in income, as a result of changes in the relative prices of imports, exports and nontraded goods.<sup>16</sup>

To illustrate these effects, suppose that a country, which we call A, increases its exports of motorcars to another country, B, as a result of depreciation. There will be a decrease in the demand for "motor-making" factors in B, and this will reduce their relative price, and cause a shift to the right in the supply schedules of other B goods

<sup>16</sup> This point has been made by R. F. Kahn, "Tariffs and the Terms of Trade" in *Rev. Econ. Stud.*, 1947-48, XV (1), pp. 14-19; and C. Kennedy, "Devaluation and the Terms of Trade," *ibid.*, 1949-50, XVIII (1), pp. 28-41.

which use the same sort of factors as the motor industry. This shift to the right in the total supply schedules for such goods is likely to be associated with a shift to the right in the export supply schedules of those of them which are exported. It is also likely to lead to a fall in the domestic prices of these goods, whether exported or not, and this in turn will lead to a shift in the B demand curves for imports. Where the income effect of such a change predominates there will be a shift to the right in the demand curve for imports and when the substitution effect predominates, there will be a shift to the left in the demand curve for imports. Thus the increase in the volume of A exports as a result of depreciation will cause shifts in both the demand curve for imports and the supply curve for exports in country B. Similar changes in factor prices, though in the opposite direction, will be set afoot in country A, so that the position of all four curves will be affected. Only if all these shifts are so small as to be negligible, will the true elasticities of supply and demand be relevant to the situation created by depreciation. While this might be so for a country whose foreign trade forms only a very small proportion of its national income, it is very unlikely to be so for a country such as Great Britain.

It may be said that the whole of our criticism of the elasticity formulae boils down to the statement that they depend on a number of simplifying assumptions, and that a similar accusation could be made against almost any proposition in economic theory. But the vital question in the application of theory to policy is whether the things which have been left out in the process of simplification are important or unimportant. Indeed a large part of the art of political economy lies in knowing what to ignore. In this case, if one accepts the factor-proportions analysis on which the pure theory of foreign trade depends, it seems that the simplifications required in order to validate the elasticity formulae are of the utmost importance, and that policy conclusions based on the formulae are far more likely to be wrong than right.

Up to now the argument of this paper has been purely negative, but it would be unfair to urge the rejection of the elasticity approach without at least trying to put something in its place. The simple theoretical apparatus, which is all I have at my command, cannot reach the same precision as appeared to have been reached in the elasticity formulae, but I think it can say a little which is of use as a general guide to policy. We can use the results which emerged from our simple models as a starting point. In these models, we found that, so long as money can be used only for the immediate purchase of goods, it is impossible to conceive of an unstable exchange market unless the market for at least one of the traded goods is also in unstable equilibrium. We also found that

if the goods markets are stable, depreciation, carried to a sufficient extent, must be capable of restoring the trade balance, though at the cost of a deterioration in the terms of trade.

For the present we concentrate on the trade balance, though it will be suggested that our theory also has relevance to the broader problem of the balance of payments. We first distinguish between the various circumstances which may cause the balance of trade to become unfavorable. For this purpose we need criteria of external and internal balance, and it is convenient to start from a point of "full employment." We define full employment as the level of employment which will maximize home-produced national income at constant prices, the value added by the setting to work of a marginal man from among the unemployed being just offset by the loss of value resulting from the frictions and dislocations of "overfull employment." The relationship between income and expenditure must obviously be an *ex-ante* one, and I take as my criteria of equilibrium that planned expenditure (public and private, for consumption and investment) on domestic goods and services plus expected sales abroad should equal the value, at current prices, of full-employment output; and that planned expenditure on foreign goods and services should equal expected sales abroad. If these plans and expectations were fulfilled, they would result in the maintenance of full employment and a neutral trade balance. We shall confine ourselves to the short period, so that we need not concern ourselves with the secular growth of full-employment output. Also, purely in order to simplify the exposition, we shall speak of two countries only, though these can be interpreted as "home" and "the rest of the world."

We can now distinguish five types of departure from equilibrium which may be, and at times have been, of great practical importance: (a) Where expected sales abroad cannot be made because international prices are out of line; (b) Where expected sales abroad cannot be made because of a fall in the foreign demand for one or more export goods; (c) Where there is inflation at home; (d) Where there is depression abroad; (e) Where there is stability in internal expenditure and a neutral trade balance, but with large idle resources. It is proposed to glance at each of these situations in turn.

*Situation (a).* We suppose that the home country is enjoying full employment, and is in *ex-ante* equilibrium in the sense described above, but that prices are such that expected sales abroad cannot be made. There will thus be a debit balance in the international account, total expenditure on domestic products will fall below the current value of full employment output, goods will remain unsold, and unemployment will appear. This primary fall in employment would tend to set up a

multiplier effect, leading to secondary falls in employment and in home-produced national income; the reduction of national income would tend to reduce the demand for imports and (together with the opposite effect in the foreign country) would tend to restore the trade balance. Normally, however, these income effects will not be sufficient fully to restore equilibrium.

We now suppose that the monetary authorities of the home country allow its currency to depreciate to the point (if such a point exists) which would restore the trade balance with a full-employment level of real income. As exports increase and domestic spending is diverted from imports to home-produced goods, national income will rise again. The income effect mentioned above, though it will continue to operate in an equilibrating direction so long as it exists, will grow progressively weaker and finally, with a return to full employment, will disappear. The problem is, therefore, whether there is a new exchange rate lower than the old which will maintain equilibrium in the trade balance when the old level of home-produced real income is restored.

Here the reasoning of our simple model of pages 285-86 can be of use. If the monetary authorities both of the home and the foreign country were to take steps to maintain the total money expenditure of their citizens at the level at which it was running before the departure from equilibrium (*i.e.*, equal to the value at current prices of full-employment output) we reproduce artificially the essential condition of our simple model. A decrease in expenditure in one sector of the economy must have its counterpart in an equal increase in expenditure somewhere else, just as it would if money could only be used, as in our simple model, for the immediate purchase of goods. In particular, any decrease in expenditure on foreign currency in the exchange market must be associated with an equal increase in expenditure on domestic products.

The impact effect of depreciation will be a cheapening of home goods in terms of foreign ones, a reduction in the volume of imports and a reduction in the demand for foreign currency. The exchange market will be stable unless this reduction in demand is associated with an even greater reduction in supply. But a reduction in the supply of foreign currency in the exchange market must mean that foreigners are spending more on their own products. This increased expenditure would be diffused over a great variety of products, and there might be most complicated substitutions between one and another, but it still remains true that there must be at least one good for which the increase in foreign demand is greater than the reduction in demand from the home country. In other words, there must be at least one good for which an increase in relative price would increase excess demand. We

thus reach the conclusion that a sufficient measure of depreciation must be effective in restoring the trade balance unless at least one of the product markets is in unstable equilibrium.

This conclusion is independent of whether traded goods are produced in both countries or not, and its validity does not depend on the assumption of constant prices made in our simple model. If depreciation produced a secondary rise in the domestic price of the home product, this would mitigate any fall that might occur in the supply of the foreign currency (since a fall can only occur if foreigners spend less on imports the lower the price) but it would in no way alter the fact that, if such a fall does occur, it must be associated with an increase in foreigners' spending on their own products. If there is a secondary fall in the price of the foreign products, it follows a fortiori that an increase in total expenditure on them must produce an increase in excess demand.

The same conclusion must obviously hold, if money expenditures are not maintained at a constant level, so long as expenditure in the home country does not increase and that in the foreign country does not fall below its equilibrium level as a result of depreciation. There is no obvious reason why foreign expenditure should fall below its equilibrium level,<sup>17</sup> but that of the home country might rise above it if depreciation brought claims for increased wages or if there were induced investment. It would, therefore, be necessary for the monetary authorities of the home country to prevent money expenditure from rising above its equilibrium level, and for the trade unions to refrain from pressing claims for wage increases which would make it impossible to regain full employment with this level of expenditure. On the other hand, the government could safely take the steps to offset any reduction in money expenditure and mop up any idle resources which might appear during the transition period.

This analysis is more general than the elasticity formula, for it can be applied to the whole foreign account. As long as the markets for services and assets, as well as goods, are stable, and as long as expenditure on all items and not merely on goods, is held constant, depreciation must correct not only the trade balance but the balance of payments as a whole.

How much importance we attach to this conclusion depends on how much confidence we have in the stability of the markets concerned. Experience affords very strong grounds for believing that the markets for services and for most forms of goods are stable. Economic analysis

<sup>17</sup> The unfavorable balance for the home country would be a favorable balance for the foreign country, and would tend to raise its income above the equilibrium level; the elimination of the balance by devaluation would thus only reduce foreign income to its equilibrium level.

and policy are both based on this assumption, and if we regulate our internal affairs satisfactorily on the assumption that we are dealing with stable markets, it seems only reasonable to make the same assumption in framing international policies.

On the other hand, the markets for commodities with low storage costs, real estate, stocks and shares, and currencies for hoarding are subject to periods of speculative instability, though these periods are generally brief. It is, therefore, quite possible that depreciation may set up price expectations in some of these markets which will lead to a period of instability, and if the latter is sufficiently strong, it may lead to instability in the exchange market as a whole. This is a strong argument against completely free exchanges, and in favor of an exchange equalization account which can be used to combat speculation. In my judgment, however, it does not seriously reduce the likelihood that, in the situation outlined above, an appropriate amount of depreciation will ultimately serve its purpose.

We can now dispose comparatively briefly of the other situations outlined above.

*Situation (b).* In this situation we suppose the same *ex-ante* equilibrium as in (a) but that the trouble with the trade balance arises not from general prices, but from a fall in the foreign demand for a particular export. This may be supposed to arise either from a change in tastes, from a change in foreign techniques which robs an export industry of its comparative advantage, or from the imposition or raising of a foreign tariff. The income effect will be as in situation (a) but there will also be a fall in the relative price of the export good concerned, which will have secondary repercussions on the whole structure of prices. Again, however, the maintenance of total money expenditure at an equilibrium level together with an appropriate amount of depreciation will restore the balance as long as the goods markets are stable.

*Situation (c).* This situation is very different from either of the two previously discussed. Here we suppose that planned expenditure on domestic goods plus expected sales abroad exceeds the current value of full-employment output. If domestic plans are carried out, expected sales to foreigners cannot be realized, not for lack of demand but because the goods will not be there to sell. If depreciation did succeed in luring some goods from the home to the foreign market, the only result would be that domestic plans would become incapable of fulfillment, and the attempt to fulfill them could only drive up prices, thus tending to nullify the effects of depreciation. The only way to cure this situation is to bring about a reduction in domestic spending plans; any revision of these plans which resulted from depreciation would be more likely to be in an upward than a downward direction, and

depreciation in these circumstances would be worse than useless.

*Situation (d).* In this situation we suppose that the home country starts from a position of equilibrium as in (a), but that sales fall short of expectations because of a fall in employment and incomes abroad. This will produce domestic income effects as in (a), but the situation will be more complicated because any reduction in the home demand for imports will be "reflected back" through a further fall in foreign income and in the foreign demand for exports. There would be a similar disequilibrating effect associated with depreciation. So far as depreciation reduces the home demand for imports or pushes exports in substitution for foreign produced goods, it will depress foreign income and bring about a secondary reduction in the foreign demand for exports. The full analysis of this situation is very complicated. It is clear, however, that (since we can no longer assume that foreign expenditure is maintained at the equilibrium level) the argument applied to situations (a) and (b) is not valid here. It is by no means certain that there is any exchange rate which will restore the balance of payments consistently with the maintenance of full employment at home. Even if such a rate does exist, depreciation will do its work only at the expense of complicating the stabilization program, of the foreign country.

Depreciation does not seem an appropriate remedy here. The best policy is clearly for the government to offset the fall in exports by creating additional spending at home (thus maintaining equality between actual spending and the value of full-employment output) and to meet the resulting foreign deficit from reserves. This is what reserves are for, and a country with adequate reserves should be willing to see them drawn on heavily in such a situation. If reserves are so small as to make some restriction of imports essential, then it would be better to bring it about directly than to rely on the uncertain working of depreciation.

*Situation (e).* This is Mrs. Robinson's "beggars-my-neighbor" case. One way of posing this is, "will depreciation turn a neutral balance into a favorable one on the assumption that there are no changes in spending except those following from the act of depreciation itself?" If we assume, with Mrs. Robinson, that depreciation will affect total domestic spending and home-produced real income only through changes in the trade balance, then the reasoning of (a) will apply, and we should expect depreciation to produce a favorable balance. If we take account, with Harberger, of the changes which are likely to be set afoot independently of changes in the balance, then the argument does not apply, and there is no certainty about the result. In any case, the creation of such a balance would set up income effects inimical to it, so that the extent of the movement would be unlikely to be large.

A slightly different way of posing the problem is to suppose that both countries have idle resources, and to ask whether depreciation would make it possible to sustain a controlled increase in domestic expenditure (unmatched by any similar increase abroad) without producing an adverse balance. Since depreciation with constant expenditure has been shown to improve the balance, there is presumably some increase in expenditure which could be sustained, consistently with the maintenance of a neutral balance, with an appropriate amount of depreciation. This would not, of course, have any deflationary effect on the foreign country.

To sum up, the first and negative part of this paper tried to show that the theory of flexible exchanges is not a good subject for analysis in terms of elasticities and marginal propensities; the number of variables is too large and the functional relationships too complicated. It has only been handled in this way with tolerable simplicity by constructing models which leave out essential features of the adjustment mechanism, and the attempt to draw policy conclusions from such models is bound to be misleading.

The second part of the paper attempts to make some generalizations based on the pure theory of foreign trade, and which might reasonably be expected to apply to real world conditions. The theory lacks the precision of the elasticity formulae, and I have no doubt that, in subtler hands than mine, it is capable of a great deal of refinement. It is, therefore, advanced only very tentatively. I feel, however, that it does at least tell us when depreciation is, and when it is not, likely to be useful. It will not cure the balance-of-payments problems attendant on domestic inflation; it is not likely to help much in mitigating the effects of a foreign depression; and it will be of very limited usefulness as a purely beggar-my-neighbor remedy. On the other hand, when used in conjunction with a controlled increase in domestic expenditure, depreciation may help in the escape from a position of underemployment equilibrium. The chief usefulness of flexible exchanges is, however, when both the countries concerned are enjoying a high level of domestic employment, but without inflation. It is here that our conclusions differ radically from those of the elasticity approach, for our analysis has shown that—on assumptions which are by no means beyond the powers of a sensible monetary and fiscal policy—depreciation is bound to cure an adverse balance. It would thus appear that we have underestimated the flexibility of the international economic system. The path to free convertibility, multilateral clearing and the removal of quantitative restrictions will certainly not be smooth, but it need not be the impassable jungle which we have sometimes been led to expect.

## OBSOLESCENCE AND TECHNOLOGICAL CHANGE IN A MATURING ECONOMY

By MARVIN FRANKEL\*

It is a frequently asserted proposition in economic literature that older, industrialized countries such as Britain suffer a disadvantage relative to newer, industrializing countries. More generally, it is argued that both individual industries and entire economies, once they have reached an advanced state of development, experience difficulty in assimilating later and more modern techniques. The time comes, in the words of Veblen, when installations, having been "placed and constructed to meet the exigencies of what is now in a degree an obsolete state of the industrial arts," and having changed little since their origin, "are, all and several, 'irrelevant, incompetent and impertinent' in the same degree in which the technological scheme has shifted from what it was when these appliances were installed."<sup>1</sup> As a result, costs are higher and productivity lower than they ideally ought to be, and plant and equipment are old and inefficient.

A variety of hypotheses has been advanced in further explanation of this state of affairs. They may be grouped conveniently, if roughly, into three categories: those which stress institutional rigidities; those based upon resource inadequacies; and those emphasizing the effects of sunk costs. For shorthand purposes, let us term these categories respectively the institutional, the environmental and the technological.

In the first category belong such common factors as labor resistance to technical change, entrepreneurial provincialism, lack of knowledge, immobilities of labor and money capital, and more generally, archaic modes of thought and patterns of action. These, it has been said, mature and strengthen as an industry or country advances industrially, so inhibiting innovation and modernization adjustments.<sup>2</sup>

In the second category, termed environmental, should be put those

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<sup>1</sup>Thorstein Veblen, writing on Britain in *Imperial Germany and the Industrial Revolution* (London, 1915), Ch. IV, "The Case of England," p. 127.

<sup>2</sup>In this category belongs a substantial part of Veblen's explanation, which runs in terms of "custom, wont and usage," for British backwardness. See *ibid.*, Ch. 2-4. Here too should be put what George W. Terborgh has called "a failure of reequipment policy"

hypotheses which posit as the cause of retardation the presence of limitational elements—limited supplies of land or other resources. So-called modern methods may require that certain factors be used in quantities or ratios that are uneconomic for some industries and areas. Thus a country which, at the inception of its industrialization is plentifully endowed with the resources necessary to modern production methods, may subsequently find that some of those resources have become scarce and expensive. Or it may find that a changing technology requires new resources that are in short supply. In either case it will adapt only haltingly and with difficulty to the new ways.<sup>3</sup>

It is with the third or technological category, which embraces effects attributable to sunk costs, that this paper will deal. The relevant hypothesis can be stated as follows: As an industry (or industrial economy) grows and adapts to changing and increasingly complex production methods, interconnections, more or less rigid, develop among its technological components—among machines, plant, transport network and raw material supplies—that make increasingly difficult the introduction into the system of new, cost-saving changes. It may then happen that the entire system becomes obsolete because, as Veblen has observed, "An adequate remedy by detail innovation is not always practicable; indeed, in the more serious conjunctures of the kind it is virtually impossible, in that new items of equipment are necessarily required to conform to the specifications already governing the old."<sup>4</sup> Unable to utilize the new production methods, the industry continues with its old ones. As a result, its costs are higher and labor productivity lower than they would be in a less "mature" industry. The old industry finds itself penalized for having taken the lead and shown the way to its young competitors in other regions.

This view seems to contradict that implicit in orthodox cost-price theory, according to which the rational entrepreneur or manager will introduce a cost-saving method either immediately or at some later date

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—lack of an adequate technique for analyzing the replaceability of equipment. "... Britain knew how to build great industries but never learned how to rebuild them." "Capitalism and Innovation," *Am. Econ. Rev.*, Proceedings, May 1950, XL, 122.

<sup>3</sup> Under this heading, in addition to explanations resting directly on absolute or relative resource shortages, fall explanations based on logistic theories of growth and, in part, those involving a frontier thesis. Implicit in the logistic function is the notion of a factor or factors in short supply that exert increasing resistance to growth; while the frontier thesis is premised upon a plentiful land supply, development and settlement of which profoundly condition production methods and other institutions. At times the dividing line between the institutional and environmental categories may be quite vague. The frontier thesis straddles both categories; and shortage of resources, whether human or natural, can result from artificial as well as natural barriers.

<sup>4</sup> Veblen, *op. cit.*, pp. 125-26. Veblen's discussion of British backwardness runs in terms of both the technological and the institutional.

when the old plant and equipment wear out. Under this latter view, and barring the presence of institutional or environmental factors, the old industry is as free as the newcomer to adjust to the changing times.<sup>5</sup>

The paragraphs that follow inquire into the technological explanation of backwardness and attempt, through reference to conventional cost-price theory, to define the conditions governing its validity. The implications of those conditions for a maturing industrial economy are then explored. Finally, some observations are offered on the extent to which the conditions prevail in practice.

### I. *Innovation Criteria*

At the outset it is necessary to distinguish between method innovation, a change in the technique for producing a given product, and product innovation, a change in product type or quality. Because abstraction from the complicating effects of product innovation will simplify analysis of our central problem, our discussion relates mainly to method innovation.

The analysis requires use of two cost categories: past or sunk outlays, and future outlays. Past outlays denote expenditures already made which are allocable to some future production period and which must be recovered from future revenues. The undepreciated portion of costs sunk in capital equipment is the principal type of such outlays, and the only type our analysis will explicitly treat.<sup>6</sup>

Future outlays denote all costs that will be incurred over some future production period: costs for labor, raw materials, inventories, repair and maintenance, etc. Also included are outlays for replacement of capital equipment or for new capital equipment which have not yet been made but which must be made over the coming years.

The total costs of any production method are represented by the sum of the past and future unit outlays associated with its use. For an old method already in use, past outlays always are some positive amount unless the cost of equipment has been completely written off. For a new method not yet adopted, past or sunk outlays are zero, and total costs are represented by future outlays.

Let us limit consideration to assets of the one-horse-shay variety.

<sup>5</sup> For a statement of this view see F. R. J. Jervis, "The Handicap of Britain's Early Start," *The Manchester School*, Jan. 1947, XVI, 116, 118.

<sup>6</sup> Some fraction of past outlays for research, training programs and advertising also fall into this classification. Strictly speaking, outlays for raw materials and inventories already in hand also are past outlays. But here the interval of prepayment is relatively short—usually less than a year—and the sums involved are not sunk in specialized equipment but are more readily recoverable. For these reasons it is more convenient to consider raw material and inventory costs as current operating costs and to treat them as if they were future outlays.

Let us further define the production period to which costs and revenues are allocated as the remaining useable life of the capital equipment of the old production method.

Given these definitions and conditions, the criteria governing whether and when to introduce a new production method are, in principle, clear enough. A change should be made only if it will result in an increase in total profit. In making the comparison between old and new methods—and neglecting the effects of taxes—the sunk outlays of the old method have no relevance; only future outlays need be taken into account. This is so because costs which are sunk continue and require payment regardless of whether the new method is adopted.<sup>7</sup> If total profit from the new method—the excess of total revenue over total cost—exceeds total revenue less future costs of the old method, then the old method should immediately be replaced. If, on the other hand, total profit from the new method falls short of revenue less future costs of the old method but exceeds revenue less total costs of the latter, then the old method should be continued in use until the fixed plant is worn out.<sup>8</sup>

These criteria apply whether a firm is young or old, whether it is in a competitive or monopolistic market situation, and whether it is publicly or privately owned.<sup>9</sup> Two further points are worth noting.

<sup>7</sup> If payments, whether to stockholders or bondholders, are defaulted, then for purposes of innovation decisions the case may be treated as if the equipment were fully amortized, i.e., sunk costs are zero. To the extent that the equipment has a positive scrap value, its cost may be treated as an addition to the future outlays of the old method or a deduction from the future (total) outlays of the new method. In the extreme case where the scrap value equals the unamortized balance, that entire balance may be treated in this way.

<sup>8</sup> In comparing the two methods, the optimum period of use for each must be determined. Under the specified assumptions, this period for the old method is the remaining useable life of its fixed capital, while for the new method it is the durability of the new fixed capital. The periods of both must then be adjusted to the same time base and all magnitudes reduced to present values. It seems desirable also to make the comparison when each method produces at its optimum rate of output. With these points in mind, the propositions in the text are readily demonstrated.

Let  $P$  = past outlays of the old method

$F$  = future outlays of the old method

$F + P$  = total costs of the old method

$T$  = total costs of the new method

$R_1$  = total revenue from the old method

$R_2$  = total revenue from the new method

Adoption of the new method will not eliminate  $P$  since these costs are sunk and continue regardless of the method used. Hence only if  $(R_2 - T) > (R_1 - F)$ —the criterion for immediate replacement—is  $[R_2 - (T + P)] > [R_1 - (F + P)]$ . If  $[R_2 - (F + P)] < (R_2 - T) < (R_1 - F)$ , then when the equipment wears out  $(R_2 - T) > (R_1 - F)$ . This is so because at this point  $F$  is augmented by replacement costs.

<sup>9</sup> Our simplified formulation—assumptions and criteria—will suffice to elucidate the problem at hand; it is not designed to explore the many complications which may arise in making replacement—innovation decisions. For a discussion of replacement criteria under more complex and more customary conditions, see George Terborgh, *Dynamic Equipment Policy* (New York, 1949), especially Chapters II, V, VI, and XI.

First, the new firm will utilize the latest, cheapest method when it commences operations and as it expands, and its rate of expansion is apt to be more rapid than that of the old firm. This method, if its profits fall short of the excess of revenue over future costs for the older method, will be one which replacement criteria deny to the old firm until such time as it must replace its equipment. Second, in the absence of factors making for an increase in demand, and other things being equal, a new method will be introduced sooner in a competitive than in a monopolized industry.<sup>10</sup> In the former, entry of new firms and expansion by old ones will result in an immediate introduction of the new method no matter how slight the cost-saving, and prices will tend to fall to the extent that cost-savings permit. In the latter, unless profits from the new method exceed the excess of revenue over future costs for the old method, there will be little tendency to change until the old equipment is worn out.<sup>11</sup>

In this orthodox cost-price approach to the problem and, to repeat, abstracting from institutional and environmental factors, it appears that both old and new firms will employ the latest and cheapest method available at the time of investment commitments. This may mean that a more modern method with lower total costs will be used by the new firm. The differences in method and in cost, regarded by some as an indication of retardation or backwardness on the part of the old firm, may persist for a period measured by the remaining usable life of the old equipment.<sup>12</sup> But then replacement will take place. If, by this time, further technical advances have occurred, the old firm should be even more modern than its rival.

During the interim when cost and equipment differences exist, the old firm, if it has not correctly estimated its obsolescence rate, will face a financial burden in the form of some fraction of the unamortized balance on its equipment. Investors will suffer accordingly, for such is the penalty for faulty (normal?) foresight. But the burden will have no counterpart in terms of current real resource costs. *Individual* investors may calculate that they would have been better off never to have

<sup>10</sup> Other things may not always be equal. Schumpeter has stressed conditions frequently present in monopoly and absent in competition which conduce to rapid technological progress. See his *Capitalism, Socialism and Democracy* (New York, 1947), Chs. VII and VIII.

<sup>11</sup> For an analysis of innovation under various market structures, see William Fellner, "The Influence of Market Structure on Technological Progress," *Quart. Jour. Econ.*, Nov., 1951, LXV, 556-77.

<sup>12</sup> Our discussion of innovation criteria was introduced with the one-horse-shay assumption. It is, of course, conceivable that through appropriate maintenance expenditures equipment can be kept in use indefinitely. However, in practice one of two things is likely to happen: (1) Variable costs will rise as the equipment ages and replacement with the new method will become profitable; or (2) continued technological advances will eventually make introduction of the new method profitable.

taken the initial gamble. But the *community* cannot for that reason regret the addition to its capital stock and the income generated by it. Unlike the private investor, it would not necessarily be better off if the investment had been postponed. On the contrary, the community might justifiably consider itself fortunate to possess, in its old equipment, an economizing production alternative not open to its young competitor.

## II. *Application of Criteria to an Interrelated Technology*

Where, if at all, in the foregoing scheme of things can a counterpart be found for Veblen's retardation thesis? Or if, in order to find support for his views, one must depart from that scheme, then with which of its assumptions must one dispense?

Veblen's conception of technology is an organic one which views the structural and functional aspects of the production process as closely interrelated. As a firm, industry or country develops and its technology becomes more intricate, interconnections proliferate which limit sharply the range of new methods it can assimilate. Not all new techniques can be utilized, but only those which can, in some sense, conform to the past. Unlike the young enterprise or newly industrializing country which is, technologically, a *tabula rasa* without any system whose contours demand conformity, modernization for the established enterprise or country is limited by the extent to which the necessary changes can be made to "fit" the existing system.

This view of things contrasts with the traditional one of the technological unit as divisible, the capital components of the production process, such as tools, machines and plants being regarded as self-contained and individually replaceable, with each item subject to separate costing and amortization. Here the technological unit relevant for a replacement or innovation decision is relatively small, whereas in the other case it is relatively large.

Where the technological unit is large, as Veblen implies it generally is, modernization may not take place as readily as when it is small. Let us visualize a simplified production process consisting, on the technical side, of a plant which includes a series of different machines each designed to perform a necessary operation. In addition certain quantities of labor and raw materials are required. Let the plant comprise past outlays and the labor and raw materials future outlays.

If now a new machine is developed, then on conventional suppositions about technology and depending upon the degree of profitability of the new machine, two alternatives are possible: Either the old machine will be replaced immediately with the new one or it will be continued in use until it no longer is serviceable. Where technological interconnections exist, however, it may prove impossible to fit the new machine into

the prevailing pattern of operations. It might be too large for the available plant space, or too heavy for present flooring, or impossible to adapt for the continuous feeding mechanism that served the other machines, or unsuited to processing the semifabricated product in the precise way required for subsequent operations. It might be the case, that is to say, that the new machine could not be introduced unless the entire production sequence or a large fraction of it were replaced.

Confronted with this situation, the enterprise would compare the new and old methods not on a component or machine basis but on an entire plant basis since only by replacing existing plant in toto could it utilize the new machine. Even if, on the machine basis, profits from the new method exceeded revenue less future cost of the old method, comparison on an entire plant basis probably would find profits from the new method below revenue less future costs of the old method; rarely would the gains resulting from improvements in a single machine suffice to warrant immediate replacement of the entire plant.<sup>13</sup>

The situation is one which calls for a delay in replacement until the existing plant wears out. Unfortunately, however, it is highly probable that the durability of each of the several machines will differ. To replace a worn-out component of the old plant with one that fits the specifications of the new would require the discard of other components of the old plant which still were quite serviceable. On the other hand, to replace the worn-out component with a duplicate of itself would entail continued use of the older method; unless several or all of the components wore out simultaneously at a later date, the cycle would repeat itself. In these circumstances, modernization would be indefinitely postponed.

At this juncture two questions arise. First, what are the implications for unit costs of an interrelated technology? If an old production method is continued in use when, in absence of interrelatedness, it would have been replaced, then costs will tend to be higher than they otherwise would be. How much higher will they tend to be? Second—really a prior question—what are the conditions, aside from the fact of interrelatedness, which determine how much higher unit costs will be?

Mathematical formalization of our problem, as developed in Part I of the appendix, points to six determining variables. They are:

$t$ , the number of components in the old method that are linked together, or the degree of interrelatedness;

$r$ , the discount rate;

$nt$ , the durability of capital (assumed the same for both methods);

$k$ , the ratio of total variable to total fixed costs of the old method;

<sup>13</sup> Except as noted in a few places, it is of no particular consequence to the discussion whether innovation affects but a single component or affects several or all of them.

$h$ , the ratio of the fixed costs of the new to the fixed costs of the old method;

$-1/\alpha = E$ , the elasticity of demand (always negative but treated in the text without regard to sign).

For any set of values assigned these variables, it is possible to determine by how much average total unit costs of the new method can fall short of average total unit costs of the old method and still leave the enterprise content with the old method. Table I shows the outcome in a number of specific instances. To illustrate with row 4, if there are 2 components, the discount rate is 5 per cent, the durability of capital 30 years, the ratio of total variable to total fixed costs 4 to 1, new capital costs are equal to old capital costs, and the elasticity of demand is 6, then the enterprise will continue with the old method as long as total unit costs of the new method do not fall below 80.1 per cent of those of the old method.

In rows 2 through 7 of the table and again in rows 9 through 14 the values assigned each of the variables are changed once. By compar-

TABLE I.—MINIMUM UNIT COST RATIO COMPATIBLE WITH USE OF OLD METHOD<sup>a</sup>

	Number of Components	Discount Rate	Durability of Capital (years)	Ratio of Variable to Fixed Costs, Old Method (ratio to 1)	Ratio of New to Old Fixed Costs <sup>b</sup> (ratio to 1)	Elasticity of Demand	Minimum Unit Cost Ratio <sup>c</sup> (per cent)
Row	$t$	$r$	$nt$	$k$	$h_1$	$E$	
1	2	.05	15	4	1	6	90.3
2	4	.05	15	4	1	6	87.0
3	2	.10	15	4	1	6	81.1
4	2	.05	30	4	1	6	80.1
5	2	.05	15	1	1	6	60.9
6	2	.05	15	4	1.50	6	84.0
7	2	.05	15	4	1	2	93.8
8	4	.10	30	1	1.50	2	18.6
9	2	.10	30	1	1.50	2	20.8
10	4	.05	30	1	1.50	2	37.9
11	4	.10	15	1	1.50	2	39.4
12	4	.10	30	4	1.50	2	60.8
13	4	.10	30	1	1	2	21.4
14	4	.10	30	1	1.50	6	20.9

<sup>a</sup> Based upon constant variable cost functions for both methods.

<sup>b</sup>  $h_1$  gives the ratio of the fixed costs of the new component, responsible for the cost-savings, to the fixed costs of the old component it displaces.

<sup>c</sup> The figures in this column express, for the conditions specified and at respective optimum outputs, the ratio of average total unit costs of the new method to average total unit costs of the old method at which the enterprise would be indifferent between the two methods.

ing the former set of rows with row 1 and the latter set with row 8, it is possible to observe the influence of each variable on the unit cost ratio. We find that increases in  $t$ ,  $r$ ,  $nt$  and  $h$  lower the ratio and that increases in  $k$  raise it. The effects of  $E$  are more complex. Increases in  $E$  may increase or reduce the ratio, depending upon the values jointly assumed by the other variables.

### III. *Some Implications of Interrelatedness*

A hypothetical example should help to illustrate the influence of the several variables and should provide a basis for better appreciating the significance of interrelatedness.

Take the case of a given industry in each of two regions, A and B. In region A the industry is old and relatively "mature," has largely saturated its market and is experiencing a comparatively slow rate of growth. In region B the same industry is in an embryonic state, has barely tapped its market potential and, as is the case with most industries in their early stages, is undergoing rapid expansion. Assume further that in both regions the industries employ the same production method—one entailing use of a fixed factor comprising several interrelated components—and enjoy the same costs and productivities.

If now there should be a cost-saving innovation which affects a single component and if, for reasons like those specified earlier, the fitting of this innovation into the prevailing set of components is impossible, so that use can be made of it only if a completely new set of related components are simultaneously adopted, then the two industries will be affected differently. The industry in region B (industry B), because it is extending its facilities, will construct those facilities to the specifications required by the innovation. It will largely bypass the complications of interrelatedness by utilizing the innovation, together with the modified auxiliary components, as additions to existing capacity.<sup>14</sup> The industry in region A (industry A), because it has need to extend its capacity little if at all, will adopt the innovation and its related components only if it can use them profitably as a replacement for its existing facilities.<sup>15</sup> Whether it can or not will depend upon the values of the several relevant variables. The table indicates that an innovation which

<sup>14</sup> It will not *completely* by-pass the complications since, unless it is starting from scratch, it will possess some facilities based on the old method. But the fewer those facilities and the more rapid its rate of growth, the smaller will be its problem. Moreover, for reasons explained later, it probably will be true that complications facing the new industry in respect solely of its old equipment will be fewer than those facing the old industry, particularly if the new industry is part of a newly developing industrial environment and the old industry a part of an established and mature industrial environment.

<sup>15</sup> This statement assumes that, for whatever reason, entry of new firms is blocked. As is developed in Section V, interrelatedness may itself constitute a barrier to entry.

offers substantial savings may be compatible with continued use of the old method.<sup>16</sup>

Assuming for the sake of simplicity that row 4 describes the present situation and that the minimum unit cost ratio is the actual ratio, the following observations are applicable:

1. Revenue less future costs for industry A is equal to revenue less total costs for industry B. The present value of future costs is identical for both industries.

2. Industry B's unit costs are lower than those of industry A by 19.9 per cent. If the innovation is labor-saving, industry B's labor productivity will be higher than A's.

3. A logical procedure for industry A would be to write off the whole of the remainder of its past investment. Let the aggregate of fixed costs—the total of outlays for the several components—of the old method equal  $C$  and assume that the components wear out at intervals equally spaced in time. Then if the timing of the innovation coincided with the wearing out of the component it would replace, the necessary write-off would amount to  $\frac{1}{4}C$ .<sup>17</sup>

4. It seems likely that in the ordinary institutional setting the industry frequently would be unwilling or unable to write off the necessary amount—in the present case a sum equal to 25 per cent of its original investment. More probably it would maintain its capital at full book value, or would endeavor to do so. Whether it succeeded would depend upon whether it faced competition from industry B in its own market, industry B's market or in third markets. Without such competition it could continue as though the innovation had never been developed, though with higher costs, lower productivity and higher prices than industry B.

5. If a capital write-down is not undertaken, if an inflated capital base is perpetuated and if, in absence of competition with industry B, prices are maintained, investors will, in effect, have shifted responsibility for their lack of perfect foresight to other factors whose returns will be lower than otherwise, and to consumers. More generally the level of output of industry A, the distribution of income and the magnitude of savings and investment will be "distorted."

<sup>16</sup> A somewhat similar situation may arise in the absence of interrelatedness, *i.e.*, where  $t = 1$ . An instance is where the new method yields its savings only at substantially higher levels of output; levels which, with the existing demand function, are unprofitable. However, this does not alter the implications of interrelatedness. In this case its effect is to intensify (rather than originate) the disparity in unit costs between the two methods that is compatible with continued use of the old method. The "net" effect of interrelatedness then becomes the difference between the unit cost ratio when  $t = 1$  and that ratio when  $t > 1$ . This point requires no important qualifications to the observations made in the text.

<sup>17</sup>  $\frac{1}{4}C$  because there are but two components one of which has been completely depreciated and the other of which has been depreciated by half.

6. If there is competition between industry A and industry B, prices will tend to fall to the level set by costs in industry B. This may result in a forced write-down of capital in industry A and a reduced return to its investors, or a lower return to other factors, or both. Industry A may then appear as a weak and struggling competitor in interregional trade, with its costs relatively high and its productivity low.

7. The crux of the difficulty to which interrelatedness gives rise is capital obsolescence. From this follow the several secondary effects mentioned above. To be sure, this difficulty arises as well in a conventional technological world where  $t$  equals 1. But the magnitude of such obsolescence generally will be considerably less than in a world ruled by an interrelated technology. In the illustration just used (row 4 of Table I), had  $t$  been taken as 1 instead of 2 there would have been no obsolescence problem since timing of the innovation was assumed to coincide with the wearing out of the relevant component. More generally we can say that when  $t > 1$  and cost and demand conditions relating to the old and new methods are such as to leave the decision-maker indifferent between them then, depending on the timing of the innovation, the amount

of obsolescence will vary from a minimum of  $C \frac{t-1}{2t}$  to a maximum of

$C \frac{t+1}{2t}$ . When  $t = 1$ , obsolescence will vary between zero and an

amount equal to the total fixed cost of the single component.<sup>18</sup> Should the timing of the innovation coincide with the wearing out of the relevant component, the *increase* in obsolescence resulting from an interrelated

technology will be given by the expression  $C \frac{t-1}{2t}$ . As  $t$  increases, this expression approaches a limit of  $\frac{1}{2}C$ .

#### IV. *Effects on the Rate of Innovation*

To answer the question, "How do changes in each of the variables affect the degree to which, with interrelatedness, innovation is retarded," it is necessary to consider the effect of such changes on (1) the "profit gap"; and (2) the "profit potential" of the new method.

Interrelatedness has the effect of requiring the new method, if it is to be introduced, to earn higher profits than would otherwise be necessary. Let the difference between required profits with interrelatedness and required profits without it be called the "profit gap." The size

<sup>18</sup> It must be remembered that for  $t = 1$  this fixed cost is smaller than for  $t > 1$ . Thus, maximum obsolescence in the former case may well be less than even the minimum obsolescence in the latter case.

of this gap may be taken, in any particular case, as a measure of the handicap imposed on innovation by interrelatedness.

The "profit potential" is simply the profit function of the new method, since this function describes the ability of the method to earn profits. Besides affecting the profit gap, changes in certain of the variables will, other things equal, alter the profit potential. A change in one of the variables might, for example, increase the profit gap but increase the profit potential also. In this case we could not determine, without further information, whether the change served to intensify or diminish the delay interrelatedness causes to innovation.

It is tempting to infer that the combined effect on the profit gap and the profit potential<sup>19</sup> of a change in any one of our six variables is similar to its effect on the minimum unit cost ratio. Thus, if an increase in a variable reduces the ratio, it might be supposed that it also would increase the delay in innovation by increasing the profit gap and reducing the profit potential. However, that is not strictly true. It is found that increases in  $r$  and reductions in  $k$  increase the profit gap and reduce the profit potential; that increases in  $t$  increase the profit gap without affecting the profit potential; that increases in  $h$  reduce the profit potential without affecting the profit gap; that increases in  $nt$  increase the profit gap but leave the profit potential indeterminate; and that increases in  $E$  generally decrease the profit gap but have an indeterminate effect on the profit potential. In the last two instances the outcome on balance is indeterminate unless the conditions of the problem are further specified. With respect to changes in the other four variables, a few words will be offered on the implications for a developing economy.

The illustration used earlier assumed that industry B, by virtue of its later start than industry A, grew more rapidly than the latter. This more rapid growth meant that a  $t$  factor of any given size would retard innovation less for B than for A. The earlier model also assumed that the two industries employed the same techniques and had the same costs. However, it is generally the case that in less industrialized regions, in contrast to more advanced regions, factor price ratios conduce to employment of simpler, direct-labor production methods. If industry B is in such a region, while industry A is in an advanced one, then industry B can be expected to use relatively little capital and capital of a relatively simple type. In this event we have another reason why the  $t$  factor—lower now for B than for A—will be less unfavorable to B.

This argument implies that B's state of technology is more rudimentary than A's and does not, therefore, reinforce the case for lower

<sup>19</sup> See the Appendix, Part II, for the relevant functions.

costs and greater competitive strength in B than in A.<sup>20</sup> But since a lower  $t$  means a smaller handicap to innovation, it does suggest that B will be able to move up the technological ladder from its existing, rudimentary position more rapidly than will A from its existing, more advanced position. It suggests, in other words, a supplementary reason for supposing that a young industry will grow faster than an older, more fully developed industry. It also helps to explain why industries in newly developing economies tend to catch up with their counterparts in more advanced economies.

Since region B is underdeveloped as compared with A, factor price ratios will conduce to the use of a less roundabout method by industry B than industry A. For this reason we should expect the ratio of variable to fixed costs,  $k$ , to be greater for industry B. With other things equal, larger  $k$  values cause a smaller handicap to innovation than do smaller  $k$  values. Industry B will, on this account, suffer a smaller handicap than mature industry A.

On the other hand, we might expect that  $r$  would be higher for industry B than for industry A because of the more limited supplies of capital available to industry B, because of a higher marginal productivity of capital in region B—hence higher opportunity costs for using capital, and because of greater uncertainty in region B as to future investment opportunities and future rates of return. This circumstance would tend to diminish the effects of any given difference in  $t$  between industries A and B.

If a generalization is possible with respect to  $h$ , perhaps it is that the effects on industries A and B will not differ significantly. Adoption of a new method usually calls for greater capital costs than did the old method,<sup>21</sup> and this probably is as true for the industry in the less developed as that in the more developed region. We may conclude provisionally that  $h$  is neutral in its impact, neither intensifying nor mitigating the effects of a difference in  $t$  between industries A and B.

The matter can be put in a different perspective by dropping the comparison between regions and industries and applying the same principles to a single industry in a single region as the industry develops and at the same time the region becomes increasingly industrialized. As

<sup>20</sup> It is certainly conceivable that B, utilizing much more rudimentary production methods than A, would find it profitable to "jump" to a much higher technological level by adopting a highly capitalistic innovation which A, because of interrelatedness, found it uneconomic to adopt. B would be aided in such a jump by a low  $t$  factor and would be hindered by adverse factor price ratios. But in general we would expect both industries to adjust to those new methods which are proximate technologically to the methods they currently employ.

<sup>21</sup> Our reference is to total capital costs, not capital per unit of output. Advancing techniques have brought with them larger optimum outputs. A rising  $h$  is therefore compatible with either rising or falling capital costs per unit of output.

the industry and the region develop, a rising  $t$  factor will pose a growing handicap to innovation. This handicap will be intensified by a rising ratio of fixed to running costs and mitigated by a falling discount rate. It will be little affected by  $h$ , which remains relatively stable. The outcome on balance cannot be determined without reference to empirical data.

#### *V. The Theory Related to Actual Conditions*

To what extent is our analysis relevant to the complex production conditions prevailing in practice? No precise answer is possible. However, conditions in certain British industries suggest that interrelatedness has been and is a factor of consequence. Supplemental considerations bearing on the relationship between technological and certain institutional factors lend force to this conclusion. Let us take up the supplemental considerations first.

The existence of interrelatedness need not presuppose a rigid and rudimentary interdependence among a series of machines, and it ought not to be regarded as a by-product only of peculiar types of fixed capital. Its compass may include any of the elements, including the institutional ones, that make up an industry's technology. Broadly construed, technology embraces far more than physical plant and equipment. It extends also to the kinds and qualities of raw material, the labor skills, the managerial know-how and the administrative organization necessary to productive activity. Proliferation of interconnections among these elements and between them and plant and equipment is as likely, perhaps more so, as growth of interconnections among elements of plant and equipment alone. A new and more efficient machine may function efficiently, if at all, only if fed raw materials different in kind or quality from those previously used. Or it may require that those materials first be subjected to preparatory processing not previously required. Again, effective use of new equipment may call for direct labor, supervisory or maintenance skills that are not readily available and development of which in adequate quantities would require much time and expense. Or it might happen that certain equipment changes are feasible only if accompanied by a switch from singleshift to multishift working, while a short labor supply makes such a switch impossible. In still other instances use of a new method might require an expansion in output or some change in product quality which firms, for reasons of cost or want of enterprise, are reluctant to attempt.

These few examples point to the very wide field over which interconnections can develop. Elements of an institutional nature in addition to those relating to physical plant and equipment may be involved, blending with and re-enforcing one another. Their effects may not be so

readily measurable as they are where a series of machines are linked together in simple interdependence, but that does not diminish their significance for innovation decisions.

A second consideration relates to the pattern of ownership. If ownership at each of the several stages of production or within each of the stages of production is fragmented, it may prove impossible fully to consider, let alone to adopt a new method. This situation may be described as one where the relevant technological unit is larger than the ownership or decision-making unit. Instances of it can be found in the British textile and steel industries, among others.<sup>22</sup>

Returning for illustration to the example used earlier, suppose that each of the interrelated components is separately owned. If then a cost-saving substitute for one of the components is developed, the user of the component in question will be unable to innovate barring cooperation from other users. In this situation at least three alternatives present themselves: (a) The user of the obsolete component could try to induce cooperation from others by offering to share his gains with them. He might do this by quoting higher prices to his suppliers and lower prices to his purchasers. However, the potential gains from the innovation would have to be large indeed to provide, after being shared three ways, the necessary incentives to change. Further, the risks would devolve mainly upon a single user, the initiating innovator, creating a formidable obstacle to action. (b) The user of the obsolete component could go into competition with those whom he formerly supplied or from whom he formerly purchased by undertaking the several related phases of production himself; alternatively, he might buy out his suppliers and/or purchasers. He then would be in a position to adopt the new method. For several reasons—lack of knowledge of the related phases of operations and their costs, lack of financing,<sup>23</sup> reluctance to make heavy outlays in face of uncertainty, pre-emption and monopolization of the related fields by others—he might well be unable or unwilling to pursue either of these alternatives. (c) Not least likely is the possibility that the new method would be given little or no consideration. Since its introduction would require solution of problems external to the firm, the firm might justifiably disregard it, just as it disregards all those events to which it has no power to adjust.<sup>24</sup> In situations of this type

<sup>22</sup> *Infra*, pp. 312-13.

<sup>23</sup> Lack of financing is important. Even if the entrepreneur is convinced on the basis of proper calculations that an innovation is economically sound, he may not be able to convince the banks or the capital markets of this; they must discount because of their uncertainty that the entrepreneur is right.

<sup>24</sup> The situation would not differ basically if innovation gains were distributed all along the line instead of being limited to a single component. Successful adjustment would remain outside the power of any single firm. Cooperation among firms and a measure of coordination among them in effecting changes still would be necessary.

there is no definite limit to the cost disparity between old and new methods compatible with continued use of the old method.<sup>25</sup>

The last few paragraphs lead to the conclusions that virtually all of the elements in the production process are susceptible to the effects of interrelatedness and that interrelatedness need not exist in any rigid, formal, measurable way to be a factor of consequence for innovation decisions. They also indicate that interrelatedness may be important for an industry even where firms fail, because of problems generated that are external to themselves, to give it explicit recognition.

The British railway system presents something of a classic case in illustration of our general thesis. As a result of a decision as to loading gauge made in the nineteenth century, tunnels and stations restrict vehicles to eight feet in width, while on the continent and in the United States the figure is between nine and ten feet with a correspondingly greater latitude in height. This condition affects not only the possible size of car and load. As early as 1900 it prevented use in Britain of the most powerful locomotives essential to maximum economy.<sup>26</sup> Veblen had reference to this situation when he spoke of "the silly little bobtailed carriages used in British goods traffic; which were well enough in their time, before American or German railway traffic was good for anything much, but which have at best a playful air when brought up against the requirements of today. Yet the remedy is not a simple question of good sense. The terminal facilities, tracks, shunting facilities, and all the ways and means of handling freight on this oldest and most complete of railway systems, are all adapted to the bobtailed car. So, again, the roadbed and metal, as well as the engines, are well and substantially constructed to take care of such traffic as required to be taken care of when they first went into operation, and it is not easy to make a piecemeal adjustment to later requirements."<sup>27</sup>

<sup>25</sup> Attainment of economies through integration of facilities and their operation on a coordinated basis is generally recognized as a major stimulus to large-scale industry and as a factor of particular importance during the formative period of industrialization. Our concern here is with instances where integration ought to have been undertaken and was not, rather than with instances where it was successfully achieved whether it ought to have been or not.

<sup>26</sup> Charles H. Grinling, "British Railways as Business Enterprises," in *British Industries*, edited by W. J. Ashley (London, 1907), p. 156.

<sup>27</sup> Veblen, *op. cit.*, p. 126. He went on to draw the not necessarily valid conclusion that, in the community's interest, the out-of-date equipment and organization probably ought to be "junked." He doubted that they would be because "it is the discretion of the business men that necessarily decides these questions, and the whole proposition has a different value as seen in the light of the competitive pecuniary interests of the business men in control." Our analysis has shown that it may be economic—in the social as well as private sense—to continue with an old, seemingly expensive method even though a new, cheaper one be available. In the case of railroads, where the *t* factor is large, the durability of capital great and the ratio of running to capital costs low, the cost disparity consistent with perpetuation of an old system doubtless is very large. If interrelatedness is internal and not external to the

Reference to developments in the British iron and steel industry during the latter part of the last century also will illustrate our thesis. In the 'sixties, 'seventies and 'eighties a number of technical changes were evolved which offered prospect of substantial economies in various phases of production. New and commercially feasible methods of steel ingot production were opened up through introduction of the Bessemer converter and the open hearth and electric furnaces; advent of the rolling mill made possible phenomenal savings in the shaping and finishing of steel products; the optimum size of the blast furnace was greatly increased; and significant fuel economies all along the line became possible. Most important, no single one of these changes would yield its potential savings in full except in conjunction with the others, and this fact made necessary a juxtaposed grouping of the several elements of the production process.

The economies attainable from such a grouping derived from several sources: from a reduction in transport and handling charges; from a fuller and more balanced utilization of plant capacity; from easier and more accurate control over product quality; and from fuel economies.<sup>28</sup> While there could be no absolute rule, concentration of coking ovens, blast furnaces and rolling mills on one site was generally found to be the most suitable arrangement. But it was more than merely placing the various kinds of plant in the same location. They had to be concentrated "in such proportions and on such a scale that the whole plant could work effectively and economically. . . . There had to be a sufficient number of coking ovens, blast furnaces and steel furnaces both to keep one another employed and to meet the requirements of the rolling mills."<sup>29</sup>

The technical changes that made possible the economies in question materialized in an era when the British iron industry already was well developed and resting solidly on the foundations of an older technology, when its rate of growth had declined to a relatively low level, and when its ownership structure had crystallized in a pattern inconsistent with the new technological need for integrated operation. As a result, in part at least, those changes had hardly begun to be assimilated by the third

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firm, and if pecuniary interests control decisions, what is apt to occur is not a failure to modernize when modernization ought to be undertaken, but rather a failure by businessmen to write down their capital (or refusal by a utility commission to allow them to do so even if they would). In the case of monopolistically controlled railways, whose rates are regulated on a cost basis, this would result in an inflated capital base, "excess" returns to owners, and unduly high charges to consumers.

<sup>28</sup> Committee on Industry and Trade, Part IV, *Survey of the Metal Industries* (London, 1928), p. 8.

<sup>29</sup> *Loc. cit.*

decade of the twentieth century.<sup>30</sup> Things appeared to be little better by midcentury.<sup>31</sup>

As a final illustration, we might compare the adjustment to the perfection of the automatic loom of the British with the American cotton weaving industry. The loom was first introduced around the turn of the century and by 1914 represented 31 per cent of the looms in operation in the United States. By 1919, the figure was 51 per cent and by 1939, 95 per cent. In Britain, by 1939, but 5 per cent of the looms were automatic and the figure is hardly greater today.<sup>32</sup> Of the few principal circumstances contributing to this outcome, it may be urged that technical interrelatedness is one. Lancashire's industry experienced its great growth during the nineteenth century. It grew little after 1900, reaching its apogee with the first world war. In contrast, textile weaving in the United States expanded rapidly after 1900, and this expansion was coupled with a locational shift from the New England States southward that augmented the opportunity for technical modernization.

Because it grew but little, Lancashire's modernization problem was and remains primarily a replacement problem. Yet introduction of automatic looms demands more than replacement of one machine by another. For a great many firms it requires redesign of the weaving shed—often its complete rebuilding, strengthening of flooring, elimination of pillars, and respacing of machinery. It calls for equipment and method changes in the preliminary processing of yarn;<sup>33</sup> in this regard it gives cause for an integration of spinning with weaving, something for which the industry, long established on horizontal lines, is not well suited. It creates a need for product simplification and for a change in the traditional practices of converters in allocating orders to weavers, so that long runs of given fabric types may be attained.<sup>34</sup> Certain of the inter-

<sup>30</sup> In 1928 the Balfour Committee observed ". . . that few British works, if any, are modern throughout in equipment and practice, with coking ovens, blast furnaces, steel furnaces and rolling mills adjacent to one another, and making full use of waste gases. Moreover, there is not infrequently a lack of balance between the productive capacity at different stages, e.g., deficient coking oven or blast furnace capacity." *Ibid.*, p. 27.

<sup>31</sup> See R. A. Brady, *Crisis in Britain* (Berkeley, 1950), pp. 203-13. These pages provide an excellent illustration of the thesis under discussion.

<sup>32</sup> R. Gibson, *Cotton Textile Wages in The United States and Great Britain, 1860-1945* (New York, 1948), p. 70.

<sup>33</sup> For example, it requires weft yarn in larger packages. Modern ring spinning equipment will produce yarn in these larger packages or in a state suitable for easy rewinding, but most of the existing spinning equipment in Lancashire will not.

<sup>34</sup> For data on use of the automatic loom in Lancashire and some of the problems to which its introduction gives rise see the Cotton Board Conference reports, *Equipment and Labor Utilization in the Cotton Industry*, 1947; *Production, Quality, Costs*, 1948; *Reequipment and Labor Deployment*, 1948. See also *Cotton Weaving*, a report of the Anglo-American Council on Productivity (London, 1950).

connections, it will be noted, are external to the firm. Their presence, together with those internal to the firm, constitute a part of the explanation for Lancashire's continued reliance on the old power loom.

### VI. Conclusion

By way of concluding, let us ask two questions: Is it the case, as Veblen alleged,<sup>35</sup> that Britain is paying a real penalty for having taken the lead and for having shown the way to countries industrializing at a later date? If she is paying a penalty, does it follow that the United States, Germany and Japan will be paying a comparable penalty at a later date?

To the first question there is a twofold answer. First, in face of a dynamic technology, interrelatedness always gives rise to an unusually heavy obsolescence problem—a problem which at best need have no deleterious effects but which, under usual conditions, can have distorting influences upon costs, prices, competitive strength, income distribution and future rates of growth. Second, where interrelatedness is external to the firm—where the technological unit is larger than the decision-making unit, as it frequently appears to be, the firm (or industry or economy) will face an additional handicap in its inability to adopt a new method when, by economic criteria, it ought to do so. These consequences, which would not arise in absence of interrelatedness, represent a real penalty on Britain for her early industrial leadership.<sup>36</sup>

As to the second question, let us note only two things: First, the effects of interrelatedness depend upon a number of variables some of which have been formalized mathematically and others of which, like the rate of growth and pattern of ownership, have been treated qualitatively. The relative importance of the several variables and their interactions determine the significance of interrelatedness, and we can expect both that importance and the mode of their interaction to vary among countries. Suggestions have been made, however, as to the probable tendencies of certain of these variables as the industry or economy matures. Second, it is well to remember that technological interrelatedness, with its effects on adaptation to innovation, is but one of many factors that condition the responsiveness of a maturing economy to

<sup>35</sup> The British have not "sinned against the canons of technology. It is only that they are paying the penalty for having been thrown into the lead and so having shown the way." *Op. cit.*, p. 128.

<sup>36</sup> This is not at all to say the British would have been better off could they have postponed their march to industrialization. Neither is it to say *categorically* that they would be better off if they did not have to reckon with interrelatedness. After all, prodigals might live longer and more happily if they were not able to live so lavishly. But surely it is reasonable to prefer the faster innovation, lower costs and higher returns which interrelatedness prevents to the slower innovation, higher costs and lower returns which it causes.

technological change. The other factors may seriously affect the role of interrelatedness one way or the other.

### Mathematical Appendix

#### I

Insight into the effects of interrelatedness on unit cost and into the conditions determining the magnitude of those effects is obtainable through formalization of the problem as follows:

- Let  $t$  = the number of components in the old method.  
 $nt$  = the durability of capital of both the old and new methods.  
 $n$  = the interval of differential durability, *i.e.*, the interval between replacements of the components of the old method. By assumption,  $n = (nt)/t$ . That is, the interval between replacements of each of the components is assumed to be the same. Allowing  $n$  to be uniquely dependent upon  $t$  and  $nt$  permits the problem to be set up with one less variable than otherwise.  
 $r$  = the discount rate.  
 $C$  = the total capital or fixed cost of the old method over the future period  $nt$ .  
 $c_1 = c_2 = c_3 = c_t$  = the cost of each of the several components that comprise the fixed cost of the old method.

$$\sum_1^t c_i = tc = C.$$

- $C^*$  = the total capital or fixed cost of the new method over the future period  $nt$ .  
 $v$  = unit variable costs of the old method (assumed invariant with output).  
 $v^*$  = unit variable costs of the new method (assumed invariant with output).  
 $q_1 = q_2 = q_3 = q_{nt}$  = the annual output with the old method.  
 $q_1^* = q_2^* = q_3^* = q_{nt}^*$  = the annual output with the new method.

If the comparison is made at a time when one of the components has worn out and requires replacement (and assuming that the salvage value of existing plant and equipment is zero), then the present value of the total of future outlays associated with continued use of the old method becomes

$$\begin{aligned} & c_1 + \frac{c_2}{(1+r)^n} + \frac{c_3}{(1+r)^{2n}} \cdots \frac{c_t}{(1+r)^{n(t-1)}} + vq_1 \\ & + \frac{vq_2}{(1+r)} + \frac{vq_3}{(1+r)^2} \cdots \frac{vq_{nt}}{(1+r)^{nt-1}} \end{aligned}$$

which upon summing and combining terms reduces to

$$(1) \quad C \left[ \frac{(1+r)^{-nt} - 1}{t(1+r)^{-n} - t} \right] + vq \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right].$$

The present value of the total of future outlays associated with adoption of the new method for the period  $nt$  is

$$C^* + v^*q_1^* + \frac{v^*q_2^*}{(1+r)} + \frac{v^*q_3^*}{(1+r)^2} \cdots \frac{v^*q_{nt}^*}{(1+r)^{nt-1}}$$

which upon summing and combining terms reduces to

$$(2) \quad C^* + v^*q^* \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right].$$

Given a demand function of constant elasticity

$$(3) \quad p = \frac{a}{q^\alpha}$$

where  $p$  = price,  $q$  = output,  $a$  is some constant and  $-\alpha$  is the reciprocal of the elasticity of demand, the respective revenue functions over the period  $nt$  become

$$(4) \quad aq^{1-\alpha} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] \quad \text{and} \quad (4a) \quad aq^{*1-\alpha} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right]$$

and the profit functions,  $\pi$  and  $\pi^*$ , are

$$(5) \quad \begin{aligned} \pi = & aq^{1-\alpha} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] - vq \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] \\ & - C \left[ \frac{(1+r)^{-nt} - 1}{t(1+r)^{-n} - t} \right] \end{aligned}$$

$$(5a) \quad \pi^* = aq^{*1-\alpha} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] - v^*q^* \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] - C^*.$$

At optimum outputs

$$(6) \quad \frac{\partial \pi}{\partial q} = 0, \quad \text{and} \quad q = \frac{v^{-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}}$$

$$(6a) \quad \frac{\partial \pi^*}{\partial q^*} = 0, \quad \text{and} \quad q^* = \frac{v^{*-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}}.$$

If now at respective optimum outputs the enterprise is indifferent between continuing with the old method and adopting the new method, we have  $\pi = \pi^*$  or, with substitutions

$$(7) \quad \frac{v^{1-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] \left[ \frac{\alpha}{1-\alpha} \right] - C \left[ \frac{(1+r)^{-nt} - 1}{l(1+r)^{-n} - 1} \right] \\ = \frac{v^{*1-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] \left[ \frac{\alpha}{1-\alpha} \right] - C^*.$$

A meaningful economic relationship is that between capital cost outlays and variable cost outlays over the period  $nt$ . For the old method

Capital cost outlays =  $C$

$$\text{Variable cost outlays} = v \left[ \frac{v^{-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \right] nt \\ = nt \left[ \frac{v^{1-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \right]$$

Let

$$(8) \quad nt \left[ \frac{v^{1-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \right] = kC.$$

Let

$$(9) \quad C^* = hC.$$

With the substitutions permitted by (8) and (9), (7) can be written as

$$(10) \quad \frac{v^{*1-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] \\ = C \left( \frac{k}{nt} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] - \left[ \frac{(1+r)^{-nt} - 1}{l(1+r)^{-n} - 1} - h \right] \left[ \frac{1-\alpha}{\alpha} \right] \right).$$

By postulating indifference between the two methods, *i.e.*, revenue less future costs of the old method equal to revenue less future (and total) costs of the new method, we are in a position to determine the maximum possible effects of interrelatedness on costs. Were the cost-savings available from the new method any greater, its profits would be greater, and it would be preferred over the old method.

When the enterprise is indifferent between the new and the old methods,

what is the ratio, in present values and at respective optimum outputs, between their average total unit costs? For the new method, average total unit cost is given by

$$(11) \quad \frac{C^* + \frac{v^{*1-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right]}{nt \left[ \frac{v^{*1-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \right]}$$

where the first term in the numerator expresses capital cost and the second term expresses total variable cost, both for the period  $nt$ , at the optimum rate of output, and in present values. The denominator gives total output for the period  $nt$  when output is at the optimum rate.

With the old method there is a cost allocation problem. Part of the outlays for components purchased in earlier years is attributable to the period  $nt$  because those components yield up services and wear out during this period. Similarly, part of the outlays for components to be purchased during the period  $nt$  are allocable to the subsequent period. Because of the assumptions of equal durabilities for all components and equal intervals of differential durability, that fraction of earlier outlays allocable to  $nt$  is exactly offset by the fraction of outlays during  $nt$  that is allocable to the subsequent period. We may simplify further by assuming that the accruals and discounts on these two sums exactly offset each other. Then average total unit cost for the old method is

$$(12) \quad \frac{C + \frac{v^{1-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right]}{nt \left[ \frac{v^{1-1/\alpha}}{[a(1-\alpha)]^{-1/\alpha}} \right]}$$

Dividing (11) by (12), substituting on the basis of (8), (9) and (10), and combining terms gives

$$(13) \quad \frac{\left( \frac{k}{nt} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] \right)^{-1/(\alpha-1)} \left( h + \frac{k}{nt} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] - \left[ \frac{(1+r)^{-nt} - 1}{t(1+r)^{-n-t} - h} \right] \left[ \frac{1-\alpha}{\alpha} \right] \right)}{\left( 1 + \frac{k}{nt} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] \right) \left( \frac{k}{nt} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] - \left[ \frac{(1+r)^{-nt} - 1}{t(1+r)^{-n-t} - h} \right] \left[ \frac{1-\alpha}{\alpha} \right] \right)^{-1/(\alpha-1)}}$$

This expression tells us, in present values and at respective optimum outputs, the ratio of average total unit costs of the new method to average total unit costs of the old method when the enterprise is indifferent between those methods. It forms the basis for Table I.

## II

### A. The profit gap:

The difference between the profit required of the new method when inter-

relatedness exists and the profit required in its absence is obtained by subtracting from the left-hand side of (7) the same expression when  $t=1$ . This gives

$$(14) \quad C \left( 1 - \left[ \frac{(1+r)^{-nt} - 1}{t(1+r)^{-n} - t} \right] \right).$$

Substituting for  $C$  on the basis of (8) gives for the profit gap

$$(15) \quad \frac{nt}{k} [a(1-\alpha)]^{1/\alpha} [v^{1-1/\alpha}] \left( 1 - \left[ \frac{(1+r)^{-nt} - 1}{t(1+r)^{-n} - t} \right] \right).$$

B. The profit potential:

This is simply the profit function of the new method. From (5a) modified by (6a), (8) and (9) we have

$$(16) \quad \pi^* = [a(1-\alpha)]^{1/\alpha} \left( v^{*1-1/\alpha} \left[ \frac{(1+r)^{-nt} - 1}{(1+r)^{-1} - 1} \right] \left[ \frac{\alpha}{1-\alpha} \right] - \frac{hntv^{1-1/\alpha}}{k} \right).$$

## A THEORETICAL FRAMEWORK FOR TREASURY DEBT MANAGEMENT

By JACOB COHEN\*

In discussing Treasury debt management, a useful approach would seem to be to pose the various alternatives open to the Treasury and then to consider their respective implications. These alternatives can best be formulated in terms of their interest costs. We shall first develop the pure interest-saving model.<sup>1</sup> Then we shall discuss the implications of this model for liquidity. Following that, departures from the interest-saving model will be studied in terms of their effects on liquidity. It is hoped that the relationships that we develop may provide a framework for discussing the major problems of debt management—the selection of the “best” kind of securities and their allocation among different investor classes.

### *I. The Pure Interest-Saving Model*

In constructing our interest-saving model, we assume a single debt operation with the Treasury aiming at a given volume of debt revenues. We rule out “printing-press money,” direct recourse by the Treasury to the central bank, or the use of compulsion by the Treasury.

There are many alternative ways of treating the transaction between the Treasury and the market. The latter could be thought of as supplying loanable funds or purchasing claims. If the latter approach is followed, there is the question as to how to treat the demand curve. Amounts of securities purchased could be a function of different nominal interest rates with each unit being sold at par, or alternatively they could be a function of different bond prices with securities being redeemable at par. The latter alternative is the one that will be followed. Our demand curve, then, is a demand for claims in terms of the prices of these claims.

If the Treasury is to minimize interests costs, it must offer holders a maximum of special features. This will reduce interest costs by increasing the demand for government securities and/or increasing coefficients of elasticity. It must at the same time be a discriminating monopolist capable of segregating markets for its debt and setting the

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<sup>1</sup> “Interest-saving” is used synonymously throughout with “interest-reducing.”

highest interest yields (lowest prices) in the most elastic markets. The pure interest-saving model thus implies debt which is transferable within a given market but nontransferable between markets.

Perhaps the most important special feature that the Treasury can confer on its obligations is that of liquidity.<sup>2</sup> In our interest-saving model we assume that all issues are redeemable on demand either at the Treasury or at the central bank at fixed prices.<sup>3</sup> Redemption prices would be so set that the redemption of issues before their maturity would never carry any penalty; the holder would always be returned his capital investment and the effective interest rate. This should make it a matter of indifference to the investor whether he is offered short-term or long-term securities.<sup>4</sup> His demand curve for debt should be unaffected by its maturity. It should then be a matter of indifference from the standpoint of the Treasury as to what maturities it sells in a particular debt operation. The Treasury, however, might prefer one maturity length over another in order to avoid bunching refunding dates. We can then assume that the Treasury offers different maturities but that the interest return is the same regardless of maturity.

We are now ready to develop the pure case of Treasury interest-saving. We shall assume that the submarkets for debt are the commercial banks and the nonbank public. These categories would appear to have the most distinctive demand curves. Moreover, debt policy questions focus on these alternatives. Our primary concern throughout this paper will be with the best kind of securities to offer the banks and the nonbank public and the best allocation of the selected issues between these two markets.

In Diagram 1 we have drawn the bank aggregate demand curve to suggest perfect elasticity of demand. We assume that the purchase of assets by the individual bank results in a more or less equivalent increase in its deposit liabilities rather than in a loss of legal reserves. Under these circumstances, at some interest return above the costs of money creation

<sup>2</sup> For a discussion of the various kinds of special features that have been offered by borrowing governments, see my "On the Theory and Measurement of Treasury Interest Saving," *So. Econ. Jour.*, Jan. 1951, XVII, 257 ff., and "The Element of Lottery in British Government Bonds, 1694-1919," *Economica*, Aug. 1953, XX, 237 ff.

<sup>3</sup> The concept of redemption at the central bank suffers from certain ambiguities. The process described is indeed redemption from the standpoint of the holder of these issues. He can cash them in any time he wishes at a price fixed in advance of his purchase. From the standpoint of the central bank which is redeeming issues, the transaction is actually more than that. The central bank acquires ownership of these issues and can presumably resell them in the market designated by the security if it so desires. In future discussions of central bank purchase at fixed prices, we shall mean the same thing by central bank "redemption" and "purchase" of issues.

<sup>4</sup> The investor under these circumstances might prefer long-terms because they avoid the necessity of continual refundings but we can assume this to be a relatively unimportant factor.

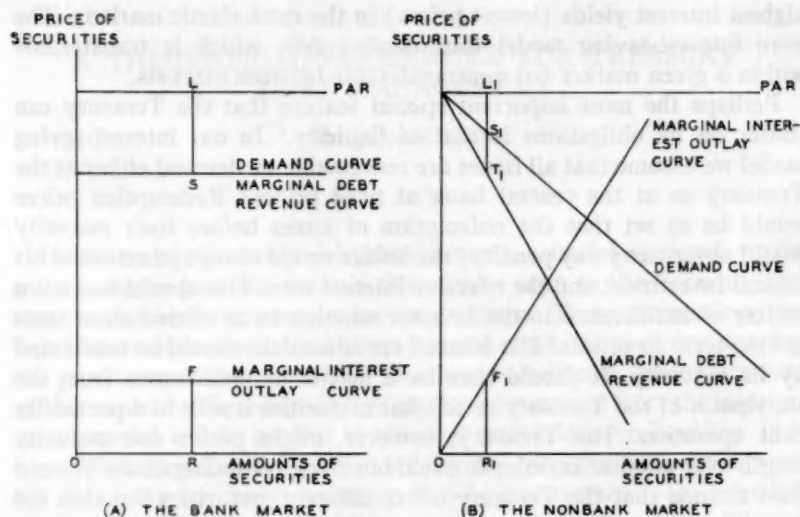


DIAGRAM 1. THE BANK AND NONBANK MARKETS FOR SECURITIES

the individual bank should be willing to buy as many government securities as are offered to it. For every dollar of government bonds that it purchases will only require the redemption of a fraction of this amount in order to secure the necessary increase in legal reserves as deposit liabilities expand. The individual and thus the aggregate bank demand curve for debt should thus approach perfect elasticity.<sup>5</sup>

We should expect considerably less elasticity in the nonbank demand curve for securities. As a matter of fact, what perhaps must be explained is why we would expect any positive response in security purchases to reductions in price. The consensus among economists seems to be that savings are relatively insensitive to interest rate changes.<sup>6</sup> When govern-

<sup>5</sup> It may be thought that our assumptions about elasticity are unduly restrictive. Even if the individual bank could not expand in a multiple of its excess reserves, could not the demand curve on the part of the *commercial banking system* approach perfect elasticity because of the possibilities of multiple bank expansion for an entire banking system? Unless some bank or banks had excess reserves to begin with, this would not be so. For without initial excess reserves, the individual bank in the banking system would not purchase a government bond if it had to cash the bond in order to meet subsequent losses in legal reserves. Perfect elasticity in the commercial banking system's demand curve would thus seem to entail perfect elasticity in the individual bank demand curve which in turn depends on the individual bank only having to redeem part of its purchases in order that it may continue to have legal reserves equal to required reserves.

<sup>6</sup> See, e.g., L. R. Klein, "Savings Concepts and Data: The Needs of Economic Analysis and Policy," in *Savings in the Modern Economy*, W. W. Heller, ed. (Minneapolis, 1953), pp. 109, 127-28; H. C. Murphy, "The Role of Interest Rates in a Changing World," *Jour. Fin.*, June 1951, VI, 238 ff; R. I. Robinson, "Monetary Aspects of Fiscal Policy," in *Fiscal Policies and the American Economy*, K. E. Poole, ed. (New York, 1951), p. 71.

ment securities are assumed to be perfectly liquid the demand for securities would presumably show a similar insensitivity. For why would the nonbank public not buy as many securities at high prices (low yields) as they would at low prices (high yields)? Complete liquidity implies that if interest yields increase, low-yielding securities could always be converted into high-yielding issues without loss.

Yet there are several reasons why we would expect a negative slope to our nonbank demand curve. The nonbank public will be induced to buy more securities at low prices than at high prices because interest yields will then be worth the cost and inconvenience of investing. More importantly, the possibilities of a future fall in yields and a subsequent capital gain are increased at lower prices.<sup>7</sup> Finally, if we do not insist on a covariation in yields in other markets, investors will shift into government securities as they fall in price.

In later discussion we shall relax our assumption that nonbank issues are completely liquid. The nonbank demand curve will then shift to the left. For now fewer governments would be purchased at any given security price because of the capital-loss effects of possible increases in market yields. The longer the maturity of the issue offered by the Treasury, the further to the left would nonbank demand lie because of the progressively greater possibilities of capital loss at any security price.

Once we are given the demand curve, a considerable amount of additional information can be read off our diagram. The marginal debt revenue curves indicate the added revenues from the sale of an extra unit of debt. We can also derive the marginal interest outlay curves once we know the demand and marginal debt revenue curves. Let us assume the sale of a one-year security in all markets. The discount from par at which a unit of securities is sold will be equivalent to an annual interest payment. In Diagram 1, such a discount in a given market is suggested by the distance from the horizontal par-value line down to the demand curve for the security ( $LS$  and  $L_1S_1$ ). Distances in turn from the demand curve to the marginal debt revenue curve measure losses on previous units as the Treasury lowers its price to sell additional units. Since the marginal debt revenue curve is identical with the demand curve in the case of the bank market, this distance is shown only for the nonbank market ( $S_1T_1$ ). The total distance from par to the marginal debt revenue lines ( $LS$  and  $L_1T_1$ ) thus equals the extra interest costs ( $FR$  and  $F_1R_1$ ) from the sale of an additional unit of securities at any given amount of security sales ( $OR$  and  $OR_1$ ) in either market.

<sup>7</sup> Under the assumption of liquidity, there is an asymmetry in the effects of changes in yields on capital gains. Redemption at a fixed price protects the investor against capital loss when yields rise. On the other hand, a fall in yields will raise the market price of higher-yielding issues.

Once the Treasury has decided on the desired amount of debt revenues, the interest-minimizing allocation of debt between the two markets follows automatically. The Treasury will allocate the debt so that marginal interest outlays for each additional dollar raised will be the same in both markets. When the ratio of marginal interest outlays to marginal debt revenues in the bank market is equal to the same ratio in the nonbank market at the same time that the desired volume of debt revenues is being secured, the Treasury will have reached the interest-minimizing allocation. If we transpose terms from one numerator to the other denominator, this formulation is seen to be equivalent to equality in the ratio of marginal interest outlays in the bank and nonbank markets to the ratio of marginal debt revenues in these two markets.<sup>8</sup>

Not only are ratios equal but the terms of each ratio in the last formulation are themselves equal at interest-minimization. Marginal debt revenues in both markets will be equal and similarly for marginal interest outlays. This is illustrated by the equality of  $SR$  and  $T_1R_1$  and the equality of  $FR$  and  $F_1R_1$  in Diagram 1. The explanation lies in the fact that the marginal debt revenues and marginal interest outlays from any allocation of debt in a given market always sum up to the face value of the security (this face value being the same in both markets).

We can also tell what prices of securities and interest yields will be set in the two markets. Security prices will be highest in the less elastic market. Thus the Treasury will sell securities in the nonbank market for  $S_1R_1$  and for  $SR$  in the bank market.

The argument can now be developed with greater generality by means of indifference curve analysis. The  $R$  curves in Diagram 2 represent indifference revenue curves. Each of these curves shows alternative combinations of bank and nonbank debt which yield the same amount of debt revenues. Curves farther to the right indicate successively higher levels of debt revenues. The successive slopes of each curve proceeding downwards reflect the behavior of marginal debt revenues in the bank and nonbank markets as allocations to the former market are increased. When relatively great allocations of debt are made in the nonbank market, marginal debt revenues there will be relatively low as compared with marginal debt revenues in the bank market. As a result, additions of successive units of bank debt will require the subtraction of rela-

<sup>8</sup> Stated algebraically, for interest minimization,

$$\frac{MIO_b}{MDR_b} = \frac{MIO_n}{MDR_n}$$

where the subscripts  $b$  and  $n$  indicate the bank and nonbank markets respectively. Transposing terms we get,

$$\frac{MIO_b}{MIO_n} = \frac{MDR_b}{MDR_n}$$

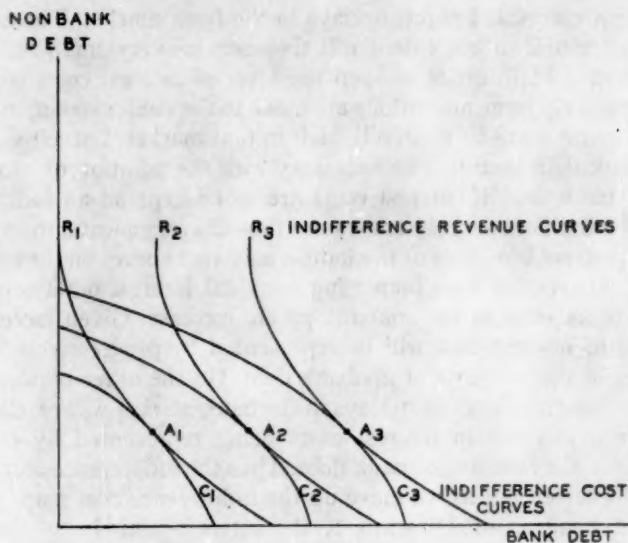


DIAGRAM 2. THE INTEREST-MINIMIZING ALLOCATIONS

tively large amounts of nonbank debt if total debt revenues are to be maintained on the same level. On the other hand, when relatively large allocations of debt are made to the bank market, marginal debt revenues from nonbank debt will be relatively high. Additions of bank debt will then require smaller reductions of nonbank debt in order to keep debt revenues constant. The curves are thus drawn convex from below.<sup>9</sup>

The indifference cost curves ( $C_1$ ,  $C_2$ , etc.) respectively represent combinations of issues sold to the bank and nonbank public which carry the same total interest costs. The successive slopes of a given curve reflect the behavior of marginal interest outlays in the two markets. When relatively large allocations are made in the nonbank market, marginal interest outlays in that market will be relatively large as com-

<sup>9</sup>We have drawn the indifference revenue curves as negatively sloping. There may also be positively sloping sections to these curves. Two factors will have to be present for positively sloping sections in the upper portions of the revenue curves. First, the nonbank demand curve must have regions of inelasticity and negative marginal debt revenues. Second, desired revenues must be such that the onset of negative marginal revenues prevents the raising of the entire amount from the nonbank public. The  $R_1$  curve has been drawn to suggest an absence of the second condition. Desired revenues are assumed low enough to permit the raising of all desired revenues from the nonbank public. The  $R_2$  and  $R_3$  curves terminate short of the nonbank axis because additional units of nonbank debt are assumed to carry negative marginal revenues. Since we assume perfect elasticity in the bank demand curve, there is not a similar possibility of positive slopes in regions of large amounts of bank debt and small amounts of nonbank debt.

pared with marginal interest outlays in the bank market. The addition of one more unit of bank debt will then require very small reductions in nonbank debt in order to keep the level of interest costs constant. When relatively large allocations are made to the bank market, marginal interest outlays will be relatively high in that market. Large reductions in nonbank debt then will be necessary with the addition of successive units of bank debt if interest costs are to be kept on an indifference level. The indifference cost curves are thus drawn concave from below.

The apparent bunching of the indifference cost curves on the nonbank vertical axis results from increasing marginal interest outlays on nonbank debt as sales to the nonbank public increase. Given increases in the level of interest cost will be represented by progressively smaller increases in the amounts of nonbank debt. On the other hand, the behavior of marginal interest outlays in the bank market will result in the same given increase in interest costs being represented by constant increases in the amounts of bank debt. Thus the indifference cost curves appear to flatten out as we move up the indifference cost map.

In the analysis of Diagram 1, the interest-minimizing allocation occurred when the ratios of marginal debt revenues in both markets equaled the ratios of marginal interest outlays. Expressed in indifference-curve terms, interest-minimization for a given level of debt revenues occurs at tangency of the relevant indifference revenue curve with some one indifference cost curve. At tangency, the ratios of marginal debt revenues and the ratios of marginal interest outlays must be equal. As would also be expected, the slopes of each curve at tangency equal unity ignoring algebraic sign. Marginal debt revenues are equal and similarly for marginal interest outlays.

The assumption of perfect elasticity in bank demand results in the tangency points of successive pairs of indifference cost and indifference revenue curves lying along a horizontal straight line as suggested by points  $A_1$ ,  $A_2$  and  $A_3$  in Diagram 2. Minimization of interest costs for successively higher levels of debt revenues are effected without change in the amount of nonbank debt but with increasing amounts of bank debt.<sup>10</sup> If the bank demand curve were less than perfectly elastic but more elastic than the nonbank curve, the successive tangency points would lie on a positively sloping line the slope of which would be such that, for each increase in total revenue, the increase in bank debt would exceed the increase in nonbank debt.

<sup>10</sup> This can also be seen from Diagram 1. The assumption of perfect elasticity in bank demand results in the ratio of marginal interest outlays to marginal debt revenues being constant no matter what allocation is made to the bank market. There is only one possible allocation of debt to the nonbank public which can yield the same ratio. Interest-minimization thus implies a unique allocation of debt to the nonbank public regardless of the amount of revenues borrowed.

This, then, is the pure interest-saving model, based on the maximum use of special features by the borrowing or refunding government. The alternate allocations of debt suggested by this model will have alternative implications for the level of liquidity and the level of interest costs. We must now discuss these implications.

## *II. Interest Costs and Liquidity: The Interest-Saving Model*

### *The Primary Liquidity Relation*

On the basis of Diagram 2 we shall develop the basic relationship between variations in interest costs and liquidity for a given level of debt revenues.<sup>11</sup> This relationship is initially described by the  $L_1$  curve of Diagram 3. We shall refer to it as the primary liquidity relation. Numerical values have been assigned to various points on the axes of the diagram in order to facilitate the exposition. Underlying these values are the following assumptions. Desired debt revenues in the given debt operation amount to \$10 billion. The bank demand curve is perfectly elastic at the security price of \$95. Five million units of securities can be sold to the nonbank public at a price of \$99 and every one dollar reduction in price will be accompanied by a five million increase in the number of units demanded.

The  $L_1$  curve begins at the amount of bank sales corresponding to the interest-minimizing allocation of debt. On the basis of our assumed values, equality of marginal debt revenues and thus interest-minimization occurs at a level of liquidity (bank borrowing) of \$8,545 million and interest costs of \$495 million. Movements down the  $L_1$  curve describe the relationship between reduced sales of issues to the banks and increased levels of interest cost. Let us say that the desired volume of debt revenues of \$10 billion are signified by the  $R_1$  curve of Diagram 2. Movements down the  $L_1$  curve are then another way of looking at movements up the  $R_1$  curve from  $A_1$  in Diagram 2. As one moves up the  $R_1$  curve, it is intersected by higher indifference cost curves implying that increased allocations to the nonbank public are associated with higher levels of interest cost. More than this, because of increasing marginal interest outlays on nonbank debt and because more units of nonbank debt are successively substituted for bank debt, given reductions in sales to the banks will be accompanied by increasing increments in interest costs. The curve  $L_1$  is thus drawn convex from below. The

<sup>11</sup> The two different senses in which we shall employ the concept of liquidity should be pointed out. When we speak of the level of liquidity or the liquidity effects of issues, we shall be referring to the effects of debt operations on the money supply and/or the volume of money-substitutes. When used to suggest a quality of issues themselves, as for example, when we speak of highly liquid issues, the term liquidity will denote their ease of redemption without loss.

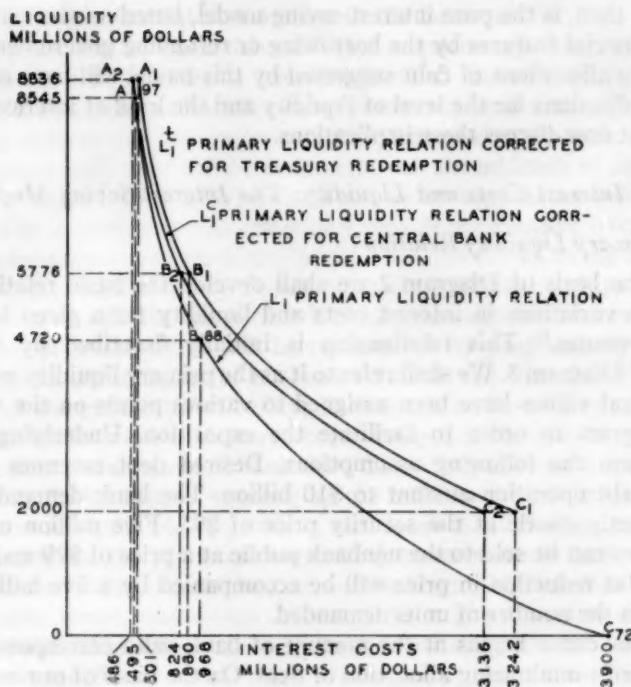


DIAGRAM 3. THE PRIMARY LIQUIDITY RELATION CORRECTED FOR TREASURY AND CENTRAL BANK REDEMPTION OF NONBANK ISSUES

degree of convexity depends on the relative elasticities of bank and nonbank demand. The more elastic the nonbank demand curve relative to the bank demand curve, the steeper the relation will be. At its extreme right, the curve ends up at the horizontal axis, indicating zero sales to the banks at interest costs of \$3,900 million.

The issues sold to the nonbank public have a high degree of liquidity because of their assumed redeemability on demand either at the central bank or the Treasury. The actual redemption of issues will have both an interest-cost and liquidity effect. These effects will differ depending on the agency of redemption. We shall explore them beginning first with redemption at the Treasury.

*The Primary Liquidity Relation Corrected for Treasury Redemption of Nonbank Issues.* Each point on the primary liquidity relation ( $L_1$ ) indicates not only the money- and interest-cost effects of a given debt allocation but also specific security prices for bank and nonbank debt. We shall derive the liquidity relation corrected for Treasury redemptions

by determining the volume of redemptions at certain selected security prices and calculating their monetary and interest-cost effects.

Let us begin with the security price of \$97 for nonbank issues (and \$95 for bank issues). These are the prices for the interest-minimizing liquidity point  $A$  on  $L_1$ . For at these security prices, marginal debt revenues will be the same on nonbank debt (\$95) as on bank debt.

Let us now assume (quite arbitrarily) that 20 per cent of the issues sold to the nonbank public at any given security price will be presented for redemption at the Treasury. The effect of such redemptions when sales are initially made at \$97 is for \$291 million of bank borrowing to be substituted for nonbank borrowing with a net increase in interest costs of \$6 million. For if desired debt revenues are to be maintained by the Treasury, the latter will now have to borrow from the banks in order to redeem the nonbank issues presented for redemption. The resultant corrections for the effects of Treasury redemption are shown by the  $A_1$  point on the  $L_1'$  relation. In similar fashion we can derive points  $B_1$  and  $C_1$ . The latter point lies to the left of the corresponding point on  $L_1$  because as soon as the price of nonbank issues falls below that of bank issues, the redemption of nonbank issues at the Treasury results in an increase in the money supply but a decrease in interest costs.

*The Primary Liquidity Relation Corrected for Central Bank Redemption of Nonbank Issues.* In treating the alternative possibility of nonbank redemption at the central bank, we assume that at any given security price the volume of redemptions at the central bank will be similar to the volume of Treasury redemption when the latter is the agency of redemption. These redemptions should have exactly the same monetary effect as redemptions at the Treasury. Instead of the Treasury issuing substitute amounts of debt to the commercial banks, the central bank will be monetizing equal amounts of nonbank debt by its purchases. The effect of central bank redemptions should then cause the liquidity points  $A$ ,  $B$ ,  $C$ , and  $L_1$  to move up to the same level as that of  $A_1$ ,  $B_1$ ,  $C_1$  respectively on  $L_1'$ .

The interest-cost effects of central bank redemptions should be quite different, however. Under Treasury redemption, interest costs on the redeemed nonbank debt are, of course, cancelled. But this decrease in interest costs is offset by the increase in interest costs on the substitute debt sold to the commercial banks. Thus, for example, at  $A_1$ , interest costs on the nonbank debt declined by \$9 million as a result of Treasury redemption. But the substitute bank debt increased interest costs on commercial bank debt by \$15 million. As a result, point  $A_1$  on  $L_1'$  showed a \$6 million net increase in interest costs as compared with point  $A$ .

Under most arrangements, the earnings of the central bank can be assumed to revert in substantial part to the Treasury. The interest costs on nonbank debt redeemed by the central bank are thus in effect cancelled also.<sup>12</sup> At the same time, since the central bank rather than the Treasury has redeemed these nonbank issues, it is unnecessary for the latter to sell additional amounts of debt to the commercial banks in order to maintain desired revenues. The essential difference between central bank and Treasury redemption is then the saving of interest costs on substitute commercial bank debt when the central bank is redeeming issues. Points  $A_2$ ,  $B_2$ ,  $C_2$  on  $L_1^c$  (which is  $L_1$  corrected for the effects of central bank redemption) lie directly to the left of  $A_1$ ,  $B_1$ ,  $C_1$  by the amounts of these savings. In Diagram 3, they are to the left by \$15 million, \$56 million, and \$106 million, respectively.

If we were to change one of our original assumptions and assume that the Treasury could borrow directly from the central bank when it (the Treasury) redeemed nonbank issues, the dissimilarities between the two cases of central bank and Treasury redemption would disappear.<sup>13</sup>

We might think of the corrected primary liquidity relations as the Treasury "housekeeping" relations. For they are of immediate significance to the Treasury, indicating the true costs to the Treasury of different initial bank-nonbank allocations. Such housekeeping relations do not reveal the entire story about the monetary effects of alternative allocations. For there are additional monetary effects from possible bank expansion, from nonbank purchase and holding of issues, and from interest costs themselves. The relation now to be developed, since it attempts to comprehend all liquidity effects, can be called the "over-all relation."

### *The Over-all Relation*

We shall now assume that all redemptions take place at the central bank. We have thus reproduced in Diagram 4 the  $L_1$  and  $L_1^c$  relations of Diagram 3. The first additional monetary effect results from excess reserves automatically provided the commercial banks by redemption of nonbank issues at the central bank. These excess reserves will be less than the amount of nonbank redemptions because of the concomitant increase in required reserves or cash withdrawals. If we make the assumption that monetization of nonbank issues takes the form of

<sup>12</sup> While not strictly correct, we shall for purposes of simplicity assume that gross central bank earnings from government debt revert to the Treasury.

<sup>13</sup> Or alternatively, if we assumed that interest earnings of central banks were true costs to the Treasury, differences between our corrected relations would again vanish.

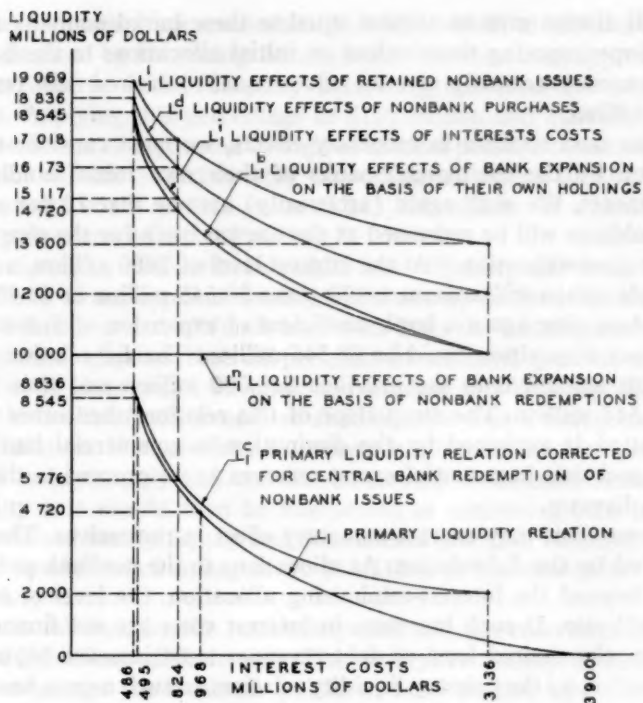


DIAGRAM 4. THE OVER-ALL RELATION

deposit liabilities, and that reserve requirements are 20 per cent, every \$100 of nonbank redemption will provide the commercial banks with \$80 of excess reserves. At minimum interest costs of \$486 million, the associated volume of nonbank redemptions will be \$291 million.<sup>14</sup> The resultant increase in excess reserves will be \$232.8 million. Assuming a coefficient of bank expansion of 5, the potential amount of assets that can be acquired by the commercial banking system on the basis of these excess reserves will be \$1,164 million. Adding this amount on to the \$8,836 million shown at  $A_1$  we secure the first point on the new  $L_1^*$  relation which thus begins at \$10,000 million. In similar fashion we can derive the two other values shown on this relation. The horizontal slope of this relation is explained by our assumptions. Twenty per cent of initial nonbank allocations multiplied by the coefficient of expansion

<sup>14</sup> All monetary effects from given bank-nonbank allocations will be plotted at the corrected levels of interest cost as these were arrived at in the preceding section on central bank redemption of nonbank issues.

of 5 will always give us a value equal to these initial nonbank allocations. Superimposing these values on initial allocations to the banking system must necessarily give us values equal to desired debt revenues of \$10 billion.

As the next stratum in monetary effects, we must consider the redemption by the commercial banks of their own initial holdings of governments. We shall again (arbitrarily) assume that 20 per cent of their holdings will be redeemed at the central bank for the purpose of further asset expansion.<sup>15</sup> At the interest level of \$486 million, a 20 per cent redemption will amount to .20 times \$8,545 million or \$1,709 million.<sup>16</sup> Assuming again a bank coefficient of expansion of 5, total possible asset acquisition could be \$8,545 million. The  $L_1^b$  relation cumulates this amount onto the previous \$10,000 million and thus begins at \$18,545 million. The steep slope of this relation when other values are plotted is explained by the diminution in commercial bank purchases and thus in potential excess reserves as we proceed to the right on the diagram.

Interest costs may exert a monetary effect in themselves. These are suggested by the  $L_1^i$  relation. As allocations to the nonbank public increase beyond the interest-minimizing allocation, the level of interest costs will rise. If such increases in interest costs are not financed by taxation, the desired level of debt revenues must increase beyond the level implied by the primary liquidity relation. In such a case, bank borrowing may also increase.

Let us assume that the corrected minimum interest cost level of Diagram 4—\$486 million—can be financed out of taxation (or desired revenues of \$10 billion). Interest costs in excess of this amount will either have to be financed out of taxation or borrowing. We shall assume that half of the excess in interest costs is financed by borrowing and that this borrowing is divided between bank and nonbank sources in the same proportion as initial bank-nonbank allocations. The problem entailed by the fact that extra borrowing will have interest costs of its own will be circumvented by assuming that they too are financed by taxation.

On this basis the new liquidity relation begins at \$18,545 million at interest costs of \$486 million. At interest costs of \$824 million, the

<sup>15</sup> These redemptions will be over and above the redemptions necessitated by the initial bank purchase of governments. Such purchases would increase the deposit liabilities of the banks or their currency withdrawals. In either case, the commercial banks would find it necessary to cash in some of their governments.

<sup>16</sup> The redemption of bank issues at the central bank should reduce true interest costs just as nonbank redemptions did. The  $L_1^b$  relation should thus begin to the left of the  $L_1^i$  relation. For simplicity's sake, however, we shall ignore this shift in the relation and assume that it also begins at \$486 million.

excess of interest costs amounts to \$338 million half of which is assumed to be financed by borrowing. At this level of interest cost, approximately 47 per cent of initial Treasury borrowing was from the commercial banks. Applying this percentage to \$169 million and multiplying it by the coefficient of expansion of 5 gives us total possible monetary effects of \$397 million. Superimposing this on the  $L_1^b$  value of \$14,720 million, results in the plotted value of \$15,117 million. In a similar way, other values can be arrived at.<sup>17</sup>

Purchases by the nonbank public have liquidity effects apart from the effects of redemptions. Such purchases may be financed out of idle cash balances or out of funds borrowed from the banking system. The  $L_1^d$  relation assumes that 20 per cent of nonbank purchases will be financed in these ways. In order to cumulate this liquidity effect onto the others already discussed we shall assume that a reduction in idle cash balances is equivalent in economic effect to an increase in the actual money supply and can thus be treated in the same fashion. The liquidity axis should then be interpreted as measuring both increases in the actual money supply and decreases in idle cash balances. On this basis the  $L_1^d$  relation begins at \$18,836 and terminates at \$12,000 million.

Issues retained by the nonbank public and not redeemed will exert a liquidity effect of their own. The moneylike quality of such issues may encourage the nonbank public to hold smaller amounts of idle cash balances. We shall assume that each \$100 of retained nonbank issues results in a \$20 decrease in holdings of idle balances. We thus calculate 20 per cent of retained nonbank issues at the selected interest cost levels in order to arrive at the  $L_1^i$  relation. This can also be regarded as the "over-all relation" since it has cumulated all the previous relations.

The height and slope of our over-all relation has been determined by the assumptions we have made about the relative elasticities of bank and nonbank demand, the ratios of bank and nonbank redemptions to initial purchases, the coefficient of bank expansion, the percentage of increases in interest cost financed by bank borrowing, the per cent of nonbank purchases financed by new money, and the cash-balances effect of purchasing and retaining nonbank issues. These are, of course, wholly arbitrary assumptions without empirical basis. Variations in them will produce an unlimited number of relations. The greater the elasticity of nonbank demand and the amount of banks' redemption of

<sup>17</sup> An alternative assumption would have considerably more of a flattening effect on the  $L_1^i$  relation. If half of the increases in interest costs were financed by a fixed percentage of bank borrowing, monetary effects would increase with increases in interest costs rather than decreasing.

their own holdings and bank asset expansion on the basis of the resultant reserves, the more negatively sloping will be our over-all relation. The greater the importance, on the other hand, of the monetary effects of interest costs, nonbank purchases, retentions, redemptions and bank expansion on the basis of the resultant increases in reserves, the more nearly positive or the more positively sloping will be the over-all relation.

While its proof may be more intuitive than empirical, we shall consider the over-all relation to be a downward sloping one, as suggested by our hypothetical example. The implications of such a relation seem more reasonable than the implications of a flat or rising relation. For a negatively sloping relation implies that a dollar of nonbank debt will have less over-all monetary effects than the dollar of bank debt for which nonbank debt is being substituted.

### III. *Interest Costs and Liquidity: Departures from the Interest-Saving Model*

#### *Illiquid Nonbank Issues*

The Treasury will depart from the interest-saving model by offering illiquid issues to the nonbank public. Issues may still be more liquid than any other paper claim but the comparison is now with the "most liquid" type of governments. Neither Treasury nor central bank redemption will be assumed to be offered the nonbank public. Liquidity will now only be provided by the marketability of nonbank issues within the nonbank sector. Bank issues are still assumed to be redeemable on demand at the central bank.

The maturity lengths of securities now become of significance for the first time. Depending on the time to maturity, an illiquid nonbank issue will have more or less liquidity. Thus the nonbank demand curve should lie the farther to the left the longer the maturity of the issue sold.<sup>18</sup> If only a single maturity is offered the nonbank public in a given debt operation, there will be a unique primary liquidity relation for each alternative length of maturity. The longer the maturity, the farther to the right on a map of liquidity relations would the appropriate relations lie.

The lower terminal point of many (if not all) of these relations would possibly occur at some positive level of bank borrowing. Assuming the raising of the same volume of debt revenues as in the case of  $L_1$  relation, the more limited demand for illiquid issues is likely to result in demand inelasticity and negative marginal debt revenues before the

<sup>18</sup> See our discussion of the nonbank demand curve, pp. 322-23 above.

debt is entirely allocated to the nonbank public.<sup>19</sup> This will be increasingly true as maturities lengthen. We should thus visualize a series of relations, each beginning to the right of the previous relation but having a lower terminal point above and to the left of each of those representing a shorter maturity.

If now we were to remove the marketability feature of nonbank issues and assume that they are nonmarketable both within and without the nonbank sector, we should reach the ultimate in illiquidity. The term nonmarketability is used strictly and does not imply the option of redemption at the Treasury before maturity. With the exception of issues with very short maturities, such nonmarketable issues would probably be unsellable except at very low prices.

The offering of many different maturities in a given debt operation would probably result in a composite primary relation lying below any of the primary relations based on the offering of relatively illiquid issues in a single maturity only. For the broadening of the nonbank market for debt by the offering of many maturities would lower the average yield (raise the average issue price) for any given amount of nonbank borrowing. In this way, the aggregate interest costs associated with a given level of money creation (bank borrowing) should be lowered. We shall refer to this composite primary relation as the " $L_2$  relation."

A composite relation will impose certain restraints on the security prices offered by the Treasury. These various maturities for marketable issues would have market prices. We would anticipate some regularity in the relationship of market yield (market price) and maturity. If long-run interest expectations are for rising rates, this would result in a "Treasury yield curve" which rose with length of time to maturity. Or (apart from interest expectations), if there is a preponderance of demand from investors wishing to maintain a high degree of liquidity the yield curve would be a rising one. The Treasury will then have to be mindful of the market pattern of rates when it sets a price on a given maturity. Variations in one security price will require consistent variations in those for other maturities as well.

There can be an indefinite number of  $L_2$  relations since at alternative patterns of rates the Treasury may limit its sale of certain maturities. Thus it may offer as many long-terms as it can hope to sell but under-allot short-term issues. Each weighting of maturities will produce its distinctive interest-cost level for given amounts of bank-nonbank borrowing and thus its own distinctive liquidity relation.

<sup>19</sup> The likelihood of negative marginal debt revenues before the debt is entirely lodged with the nonbank public is increased by the sale of illiquid rather than liquid issues in the same way that this likelihood is increased by increases in the volume of desired debt revenues (see footnote 9, above).

It is of some importance to decide how the primary and over-all relations based on illiquid nonbank issues compare with those based on liquid issues. Certainly we should expect the  $L_2$  primary relation to lie above and to the right of the  $L_1$  relation. For the composite demand curve for illiquid issues of various maturities would lie to the left of the demand curve for a perfectly liquid issue on the basis of which the  $L_1$  relation is constructed.

It is possible, however, that the *demand to hold* securities may be the same whether liquid or illiquid issues are sold. Allowing for the redemptions made possible by liquid issues (and which by assumption are not possible for illiquid issues), the demand curves to hold may be identical or be greater for illiquid issues. Just how the nonbank demand to hold for illiquid issues compares with the demand curve to hold for liquid issues has been a major issue in debt management. In Section IV of this paper we shall indicate some of the arguments that have been advanced for and against there being a greater demand to hold for illiquid issues.

#### *Illiquid Bank Issues*

As our next departure from the interest-saving model, let us consider the implications of variations in the liquidity of bank issues. Up until now, they have been assumed to be redeemable on demand at fixed prices at the central bank. They were thus equivalent to legal reserves. Let us now assume that bank issues provide only an eligible collateral for loans from the central bank. Given an aversion to central bank borrowing and its direct variation with the amount of bank borrowing, the bank demand curve might shift to the left and depart from perfect elasticity. It would probably begin at some higher interest yield than the previous bank demand curve and become progressively less elastic. The essential modifications in our liquidity relations would not appear to be substantive ones, however. Assuming the same alternative types of nonbank issues, the primary liquidity relations should now begin at lower levels of money supply and higher levels of interest cost. The reason for this is that modifications in the bank demand curve will result in minimum interest costs being associated with larger allocations to the nonbank public and higher rates of interest on both bank and nonbank debt. The new liquidity relations should continue downward and to the right of the former relations. The relations involving illiquid nonbank issues should terminate at the same level of liquidity as for the primary  $L_2$  relation but at higher levels of interest cost. In the case of liquid nonbank issues, the liquidity relation should have the same terminus as  $L_1$ . For at zero sales to the banks (as this terminal point implies) the assumed increases in bank interest costs will be ineffective.

This revised relation will then be shorter and steeper than the former  $L_1$  relation.

We have now discussed the major choice confronting the Treasury in choosing a liquidity relation. It may emphasize the sale of liquid issues or it may stress illiquid issues. In addition, there is the question of just what liquidity point (interest structure) to select once a decision has been made about the liquidity relation. These issues have been extensively debated in recent years. In the next section we shall attempt to formulate this debate in terms of our analysis. This will also provide a convenient summary to our argument.

#### IV. *The Issues in Debt Management*

The Treasury was criticized for many years, down to the time of the famous "accord" with the Federal Reserve authorities in 1951, for selecting liquid issues and setting low rates on these issues. In terms of our analysis, it was criticized for selecting low-lying primary relations and choosing points up and to the left on these relations. It was suspected that these decisions were motivated by a concern for interest costs. As a consequence, debt management was said to have been responsible for undesirable liquidity effects.

The liquidity relations selected by the Treasury in this period can be described as a mixture of the two basic types of relations on which we have built our theoretical analysis. We can think of them as  $L_2$  relations modified by some of the characteristics of the  $L_1$  relations. Thus non-marketable issues (then and now) have provisions for redemption at the Treasury at the option of the holder before maturity and thus have much of the liquidity of  $L_1$  issues. They are not completely liquid, however, because of the various waiting periods before redemption and the interest-rate penalties against early redemption. The marketable part of the debt may be viewed as  $L_2$  issues having a high degree of liquidity because of fairly rigid support of the prices of marketable issues by the central bank.<sup>20</sup> We suggest the type of issues offered by the Treasury by the  $L_1$  relation of Diagram 5

The rigid support given to market prices by the central bank tended in general to stabilize the market yield structure prevailing at the outset of the second world war. The liquidity point on a given liquidity relation is automatically selected when yields on new issues are set according to the prevailing structure of yields. Sales to the nonbank public are then determinate and so are residual sales to the banks. Total interest costs follow from this allocation and from the yields paid in the

<sup>20</sup> During the period under discussion, the degree of support underwent various modifications. Our analysis here and elsewhere will involve an obvious simplification of actual debt history.

two markets. We represent the money supply and interest-cost effects of the liquidity point selected by the Treasury by point *A*.

Our theoretical framework was constructed on the basis of a single debt operation. Redemptions at the Treasury or the central bank were assumed to be a portion of issues sold during the same debt operation. More realistically, in the course of a given debt period, issues sold in previous debt operations may be presented for redemption at the Treasury or sold to the central bank. The moving up of liquidity relations as a result of the unloading of "old" issues will probably be more significant than the moving up effects of redemption or sale of new issues. This is increasingly true as the debt outstanding grows and comes to constitute an ever larger multiple of the current debt operation. Certain inherent contradictions resulting from the mixing of  $L_2$  and  $L_1$  features tended to increase the rate of redemption of old marketable issues in the period under consideration. Unqualified central bank support was given to a term-structure of yields which rose with time to maturity. This encouraged the sale to the central bank of long-term issues as their term to maturity shortened. Moreover, bank and non-bank markets were not rigidly stratified. This resulted in nonbank holders "free-riding" by selling issues to the commercial banks whose purchase the banks had originally financed.

The sale of liquid issues runs the risk of some outside shock occurring which will precipitate a wholesale unloading by the nonbank public. For a considerable period after issues are sold, there may be a strong demand to hold issues. But once higher prospective returns develop in other markets, nonbank holders will be tempted to redeem issues acquired in past and current debt operations. This is indeed what happened in the postwar period. The  $L_1^{ct}$  relation of Diagram 5 suggests the resultant moving up of the primary relation. The fact that liquid issues quite possibly postponed monetary effects until the end of the war, was not regarded by critics of debt management as an adequate defense of low-lying primary relations in the war period. It was just in the postwar period that a containment of liquidity effects was most necessary and desirable.<sup>21</sup>

As would be suggested by our framework, the moving up of the primary relation exerts an opposite effect on interest costs and money creation. The yields paid by the Treasury on nonbank debt exceeded those paid on bank debt. The sale (redemption) of nonbank issues resulted in either increased holdings by the central bank or substitute borrowing from the commercial banks. In either case, true interest costs must have declined. The liquidity point *A* thus moved upward to the

<sup>21</sup> Cf. Woodlief Thomas, "Lessons of War Finance," *Am. Econ. Rev.*, Sept. 1951, XLI, 628.

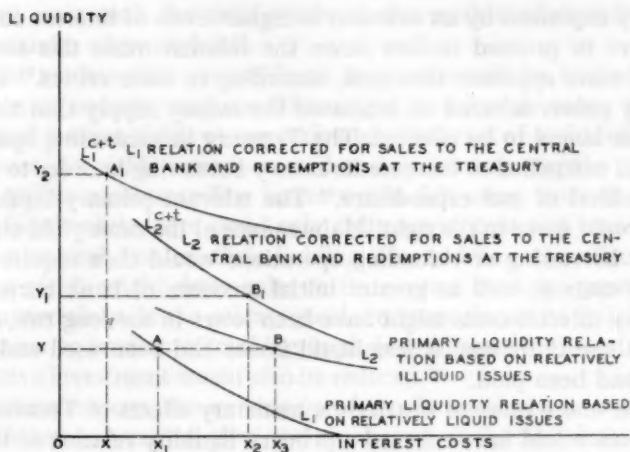


DIAGRAM 5. TWO ALTERNATIVE DEBT POLICIES COMPARED

left<sup>22</sup> to a position such as suggested by  $A_1$  on  $L_1^{c+t}$ . It would seem possible, then, that the Treasury selected liquid issues because of the interest-saving effect of nonbank redemptions. This is not likely, however. The Treasury could have secured the  $A_1$  level of interest costs more directly by selling illiquid issues to the nonbank public thus forcing itself to borrow larger amounts from the banking system. A more reasonable hypothesis would seem to be that the Treasury selected a low primary liquidity relation in order to reduce effects on the money supply within the limits set by the prevailing yield structure. Since the primary relations are downward sloping, it is of course true that selection of a low primary relation also meant that interest costs were simultaneously being reduced for the selected level of liquidity. Interest costs were not being absolutely minimized, however. The interest-saving intentions of the Treasury are perhaps most clearly revealed by its adherence to the prevailing market structure when it conducted intensive borrowing operations. If it had not adhered to this structure it could have moved even farther down the selected relation and have been responsible for even smaller increases in the money supply. That this was not done is

<sup>22</sup> The only exception to this results from imperfect stratification of bank and non-bank markets. When the nonbank public sold its holdings to the commercial banks, the monetary effect was present without the interest-cost effect. To this extent the  $A$  liquidity point moved straight up rather than to the left. The same would be true of any other liquidity point, except that liquidity points indicating initially higher yields on bank than on nonbank debt would now move straight up instead of moving to the right. The removal of stratification is thus analogous in its effects on the liquidity relation to that of Treasury redemption as compared with central bank redemption of nonbank issues only in a more decided way. (See Diagram 3.)

probably explained by an aversion to higher levels of interest cost.

Failure to proceed farther down the relation made this saving of interest more apparent than real, according to some critics.<sup>23</sup> For the liquidity points selected so increased the money supply that the price level was bound to be affected. The Treasury in succeeding operations thus was compelled to increase its money borrowing in order to finance a given level of real expenditure.<sup>24</sup> The relevant primary liquidity relation would move to the right. Maintenance of the same yield structure on new borrowing or refunding operations would then require higher interest costs as well as greater initial amounts of bank borrowing.<sup>25</sup> Treasury interest costs might have been lower in the long run, according to these criticisms, if less liquid issues had been used and higher yields had been paid.

Those who wished to restrict the monetary effects of Treasury debt operations would have selected a primary liquidity relation of the type suggested by  $L_2$  in Diagram 5. The subscript indicates that it would have been more closely akin to the  $L_2$  relation of our theoretical analysis than to the  $L_1$  relation. Of the many possible variants of composite  $L_2$  relations, it would probably have weighted long-terms more heavily than short-terms. Its initial distance above the  $L_1$  relation of Diagram 5 is explained by the relative illiquidity of the issues offered. Many degrees of illiquidity could have been offered in the place of rigid support. There could have been support with adjustable "pegs," market stabilization, or just sufficient support to maintain orderly markets.<sup>26</sup> Orderly markets would have called for the stabilizing of key issues for uncertain durations. Finally, there could be complete illiquidity which would obtain if the central bank not only failed to support securities at any

<sup>23</sup> See, for example, Lester V. Chandler, "Federal Reserve Policy and the Federal Debt," *Am. Econ. Rev.*, Mar. 1949, XXXIX, 427.

<sup>24</sup> Under certain circumstances it is conceivable that desired debt revenues would not increase. Price-level increases would raise the level of money national income. Tax receipts would then rise. If the income elasticity of tax receipts were sufficiently high, desired debt revenues would actually decrease. For this to happen, however, the income elasticity of tax receipts would have to exceed the income elasticity of government expenditures by at least the initial ratio of government expenditures to taxes. This follows from the necessary excess of government expenditures over tax receipts for there to be a deficit in the first place. With a given change in money national income then, the same percentage change in taxes and expenditures would actually result in a greater absolute change in expenditures and thus a greater deficit.

<sup>25</sup> The extent to which bank borrowing and interest costs respectively increased would depend on the repercussions of a higher money national income on the nonbank demand curve for governments. The greater the increase in demand, the greater the increased allocation to the nonbank public at prevailing yields and the greater the increase in interest costs and the smaller the increase in necessary bank borrowing.

<sup>26</sup> Cf. Robert V. Roosa, "Integrating Debt Management and Open Market Operations," *Am. Econ. Rev.*, Proceedings, May 1952, XLII, 218 ff.

time but even took the selling side of the market when inflationary circumstances warranted it.<sup>27</sup>

Let us assume that central bank support would be limited to the provision of orderly markets. This might be taken to mean the supporting of market yields at approximately the level of yields offered on new issues. As yields were increased on new issues under the proposed policy, prices of outstanding issues would fall away from their issue prices. Nonbank holders could only shift out of debt and into cash by taking a capital loss on their holdings. In addition, the effect of increasing yields might deter sales to the central bank by generating a vague uncertainty. If the private yield structure should be "sticky," the advantages to lenders of cashing in their holdings for the purpose of alternative investment would also be reduced.<sup>28</sup>

For these reasons, the moving-up of the  $L_2$  relation was expected to be much less substantial than the moving up of the  $L_1$  relation. Some moving up in the  $L_2$  relation was probably regarded as inevitable. Certain nonbank holders of marketable issues would be willing to take capital losses. Holders of nonmarketable issues might actually be encouraged to redeem as market yields rose. But on the whole, holders of marketable issues were expected to be deterred from shifting out of debt.

The higher the level of yields offered on new issues, the greater would be potential capital losses and thus the greater would be the locking-in of outstanding issues. Thus the farther down on the  $L_2$  relation that the selection of the liquidity point was made, the smaller would be the moving-up effects. For this reason the  $L_2$  relation corrected for nonbank redemptions and sales to the central bank— $L_2^{c+t}$ —is drawn as a steeper relation than  $L_1^{c+t}$ .

The payment of higher yields as suggested by the critics of debt management might have taken the form of selecting a liquidity point such as point  $B$  on the  $L_2$  relation. It represents the same initial allocation between the banks and the nonbank public as was secured by the use of the  $A$  liquidity point. The sale of the same dollar volume of debt to the nonbank public when illiquid rather than liquid issues were offered would necessarily require the payment of higher yields. The limited moving-up of this point as a result of illiquidity and higher yields is suggested by  $B_1$  on  $L_2^{c+t}$ .

If the proposed policies were effective, they would have reduced the volume of excess reserves automatically made available to the banks

<sup>27</sup> Statement of Milton Friedman, *Hearings before the Sub-committee on General Credit Control and Debt Management, Joint Committee on the Economic Report, 82nd Cong., 2nd sess., March 10-31, 1952* (Washington, 1952), p. 688; Milton Friedman "Comments on Monetary Policy," *Rev. Econ. Stat.*, Aug. 1951, XXXIII, 186 ff.

<sup>28</sup> Cf. James Tobin, "Monetary Policy and the Management of the Public Debt: The Patman Inquiry," *Rev. Econ. Stat.*, May 1953, XXXV, 122.

whenever the nonbank public sold issues to the central bank. Restrictive policies were also aimed at curbing bank expansion of credit on the basis of the banks' own holdings of governments. This would have been done by removing support prices from bank issues as well as nonbank issues and by offering increased yields on new issues. If private yields showed rigidity, this would have been an additional factor reducing the attractiveness for banks of cashing in their holdings. By lowering reserve requirements or through limited open-market operations it would still be possible to have banks play the role of residual lender to the Treasury.

In sum, the proposed policies would have attempted to minimize the over-all liquidity relation and to choose a satisfactorily low level of liquidity on this relation by substituting relatively illiquid bank and nonbank issues for liquid issues and by paying higher yields. According to one school of thought, relatively small increases in yields combined with only the provision of orderly markets for securities, would have had the effect of securing substantial reductions in the money supply.<sup>29</sup>

It is not certain, however, that these policies would have succeeded. For it is possible that rising yields would have occasioned expectations of further increases and thus have encouraged sales to the central bank despite the incurrence of capital losses. Uncertainty and confusion might have disappeared after the market had adjusted itself to flexibility. The private yield structure may be much less rigid when the employment of devices equivalent to higher interest rates is considered.<sup>30</sup> As a result, the over-all liquidity relation based on relatively illiquid issues might have been above one based on liquid issues. While higher yields would have had the effect of moving-down the over-all relation, if the moving-up effects of illiquid issues had been substantial enough, the over-all level of increase in the money supply could have exceeded the level based on lower yields on liquid issues. The period after the Treasury-Federal Reserve accord offers a somewhat limited test of this issue. For the subsequent modifications of policy were relatively moderate.<sup>31</sup> And yet, in the twelve-month period after the accord, the increase in the money supply greatly exceeded the increase for approximately nine months before the accord.<sup>32</sup> Analysis of this increase would indicate that slightly falling market yields did not succeed in locking-in the nonbank holdings of life insurance companies and other major in-

<sup>29</sup> *Ibid.*, pp. 122-24.

<sup>30</sup> *Loc. cit.*

<sup>31</sup> Cf. *Report of the Subcommittee on General Credit Control and Debt Management of the Joint Committee on the Economic Report, 82nd Cong., 2nd sess.* (Washington, 1952), pp. 16-18; Henry C. Wallich, "Recent Monetary Policies in the United States," *Am. Econ. Rev. Proceedings*, May 1953, XLIII, 36.

<sup>32</sup> *Report of the Subcommittee, op. cit.*, pp. 16-18.

stitutional holders.<sup>33</sup> Nor did they prevent a considerable expansion in bank loans and investments although this expansion was related to variations in the monetary gold stock rather than to central bank absorption of governments.<sup>34</sup>

It would be maintained by some that issues were not made sufficiently illiquid and that yields did not advance high enough to curb monetary effects. To curb increases in the money supply, interest rates should advance just as far as is necessary to achieve the desired reduction without any consideration at all to the effect on the prices of government securities.<sup>35</sup> Primary liquidity relations which began far to the right and liquidity points far to the right on these relations would have been selected if necessary under this policy. Desired debt revenues could then have been largely secured from the nonbank public without any moving-up effects.

Implicit in this analysis is some assumption about the lower terminus of liquidity relations based on illiquid issues. It is apparently assumed that the  $L_2$  relations will terminate at the horizontal interest cost axis or close to this axis. In this way, desired debt revenues could be secured in the main from the nonbank public as long as the Treasury were willing to pay the necessary interest return. It is possible, however, that the selecting of highly illiquid issues would result in relations that not only began far to the right but terminated at positive levels of bank borrowing. Negative marginal debt revenues might make it impossible to lodge most of current borrowings or refundings with the nonbank public.<sup>35a</sup> In this way residual allotments to the banking system would be increased. The failure of the relation to move up would be somewhat neutralized by the higher initial allocations to the banking system that it made necessary.

If we grant the implicit assumption that the primary liquidity relations for illiquid issues will terminate close to the horizontal axis, then another difficulty would seem to present itself. Extensive movements to the right, if necessary to curb monetary effects, would cause interest costs to increase sharply. If no weight is given to such interest costs then the argument would seem to be a logical one. But interest costs

<sup>33</sup> Statement of C. R. Whittlesey, *Hearings before the Sub-committee on General Credit Control and Debt Management*, *op. cit.*, pp. 699 ff.

<sup>34</sup> *Ibid.*, p. 710.

<sup>35</sup> Friedman, "Comments on Monetary Policy," *op. cit.*, p. 187; *Hearings*, *op. cit.*, p. 732.

<sup>35a</sup> Our analysis would apply even if securities were always sold at par. Although marginal debt revenues would always be the same positive value in this case—equal to the par value of the security—marginal interest outlays would increase with increased allocations to the nonbank public because of increasing nominal interest rates. As a result, at some allocation of debt, marginal interest outlays might exceed marginal debt revenues. The sale of additional nonbank debt at par would then cost more in terms of annual interest costs than such debt would bring in. This would be a similar situation to negative marginal revenues in the sale-below-par case.

do have economic effects apart from the "housekeeping" problems they entail for the Treasury. Interest costs may exert incentive and distributional effects because of the tax payments they occasion. Distributional effects should allow for the increased transfers vis-à-vis the private debt occasioned by higher levels of public interest cost. Unless given reductions in the money supply were always "worth," in terms of their economic effects, increasing increments in interest costs,<sup>36</sup> the optimum liquidity point would obviously not be the same point as when this is determined by consideration of the economic effects of liquidity only.

In view of these questions, rational debt management would seem to require more information than is presently available. We would have to know the various over-all relations yielded by the various alternative types of primary relations. We would have to know the economic effects of liquidity and interest costs respectively. This would involve knowing the relationship between variations in the money supply and the price level<sup>37</sup> and the economic effects of changes in the price level. We would also have to know the alternative distributional effects of alternative allocations of debt between the bank and nonbank public. Only when we had such knowledge would it seem possible to select that primary relation and that structure of yields that best approximated the optimum liquidity point.

The problem of policy is also complicated by questions of feasibility of schemes of direct control of monetary effects. Thus supplementary reserve schemes in conjunction with relatively liquid issues might tend to depress the over-all relations as compared with relations based on less or wholly illiquid issues without such controls. For it might then be possible to depress the primary liquidity relations by offering liquid issues at the same time that the significant moving-up effects of bank expansion were being curtailed. The optimum liquidity point would doubtlessly be quite different under these circumstances.

The need for greater knowledge about the effects of alternate policies has probably abated in recent years because of a weakening of inflationary pressures. The problems of debt management are likely to be most acute in periods of inflation. At such times, the selecting of liquidity points to the left on the liquidity map will be criticized for their over-all liquidity effects. In periods of relative stability or recession, on the other hand, low interest costs and high liquidity may both be quite desirable objectives.

<sup>36</sup> It is of course entirely possible that this should be so. But so far it would not seem to have been proven or disproven by empirical analysis.

<sup>37</sup> The short-run relationship between variations in the money supply and the price level was much debated after the postaccord experience of considerable price stability (cf. statement of C. R. Whittlesey, *op. cit.*, pp. 708-9; *Hearings, op. cit.*, pp. 715 ff; *Report of the Subcommittee, op. cit.*, p. 19).

## A GENERAL EQUILIBRIUM ANALYSIS OF EXCISE TAXES

By PAUL WELLS\*

This paper will present a theory of the incidence of both the burdens and benefits an excise tax exerts in a two-person, two-factor, two-commodity, perfectly competitive world.<sup>1</sup> Although this attempt may appear to be unnecessarily modest, excise tax theory should be clear on this first step before additional complicating considerations are introduced to make the analysis more realistic. The main conclusions of this paper are: (a) that an excise tax exerts both a burden—an aspect of excise taxes long recognized and much discussed in the literature<sup>2</sup>—and a benefit, an aspect of excise taxes that has been little discussed in the literature; and (b) that these burdens and benefits fall on individuals as buyers and sellers of goods and services, and that the degree to which the burdens and benefits of an excise tax spread out from one individual or group of individuals to another individual or group of individuals will depend upon the preference functions of all individuals, the asset structure of all individuals, the tax and expenditure policy of the taxing agency, and the nature of the transformation functions of the commodities produced. We shall demonstrate these conclusions for our two-dimensional world, and then consider how far it is possible to go in generalizing from it.

\*The author is a graduate student in economics at Stanford University. He is indebted to Dr. John Fei, Massachusetts Institute of Technology, and to Professor Elmer D. Fagan, Stanford University, for helpful suggestions concerning the analysis contained in this paper.

<sup>1</sup>Recent attempts, along much different lines from the one to be followed in this paper, to subject incidence theory to a general equilibrium analysis have been made by E. R. Rolph, "A Proposed Revision of Excise-tax Theory," *Jour. Pol. Econ.*, Apr. 1952, LX, 102-17, and "A Theory of Excise Subsidies," *Am. Econ. Rev.*, Sept. 1952, XLII, 515-27; R. A. Musgrave, "On Incidence," *Jour. Pol. Econ.*, Aug. 1953, LXI, 306-23, and "General Equilibrium Aspects of Incidence Theory," *Am. Econ. Rev.*, Proceedings, May 1953, XLIII, 504-17; J. A. Stockfish, "Excise Taxes: Capitalization-Investment Aspects," *Am. Econ. Rev.*, June 1954, XLIV, 287-300. See also J. F. Due, "Toward a General Theory of Sales Tax Incidence," *Quart. Jour. Econ.*, May 1953, LXVII, 253-66.

<sup>2</sup>Cf. F. Y. Edgeworth, "The Pure Theory of Taxation," *Econ. Jour.*, Mar. 1897, VII, 46-70; A. Marshall, *Principles of Economics*, 8th ed. (New York, 1949), pp. 413-15; E. D. Fagan, "Tax Shifting and the Laws of Cost," *Quart. Jour. Econ.*, Aug. 1933, XLVII, 680-710, and the literature there cited.

### I. The General Equilibrium Model

In formulating the General equilibrium model to be used,<sup>3</sup> we need, in addition to the assumptions usually required by perfect competition, the following assumptions: (a) Two people, a farmer called *A* and a worker, *B*; (b) Two factors of production: land and labor, fixed in supply and completely owned by *A* and *B*; (c) The ratio of land to labor owned by *A* exceeds the land-labor ratio of the economy;<sup>4</sup> (d) Two products: food and clothing, each of which requires the use of both land and labor for its production; (e) The production functions for food and clothing are homogeneous and of the first degree; (f) The production of food is a relatively land-intensive industry and the production of clothing a relatively labor-intensive industry.

With these assumptions, we can develop the following functions:

1. A production-possibility function between food and clothing (*PP'* in Figure 1).

2. A division-of-output function (*KL*). This function states the ratio in which output is divided between *A* and *B* for all possible output combinations to be found in *PP'*, and can be used to show the degree to which the division of output between *A* and *B* changes as a consequence of, say, the levying of an excise tax on one of the two commodities. If we let the radial line *OJ*<sub>1</sub> be an index of output *J*<sub>1</sub>, then the intersection of the *KL* function with this index states the ratio *OD*<sub>1</sub>/*D*<sub>1</sub>*J*<sub>1</sub> in which output *J*<sub>1</sub> will be divided between *A* and *B* in payment for the services of land and labor given up to the productive sector by *A* and *B*, with *OD*<sub>1</sub> of *OJ*<sub>1</sub> output going to *A* and the remainder, *D*<sub>1</sub>*J*<sub>1</sub>, going to *B*. From the shape of the *KL* function we notice that *A*'s share of output increases, and *B*'s decreases, as more food and less clothing are produced. This follows from assumptions (c) and (f). Assumption (f) requires the price of land to rise relative to the price of labor as more food and less clothing are produced; and, as a consequence of assumption (c), *A*'s share of output will increase and *B*'s decrease. The curvature of *KL* at, for example, any point *D*<sub>1</sub>—that is, the degree to which the division of output would change as a consequence of a small change in the pattern of output from that shown by *J*<sub>1</sub>—depends upon: (a) the absolute difference in the proportions in which the two factors are combined, respectively, in the two industries when the output is *J*<sub>1</sub>. The greater this difference is, the greater must be the change in relative factor prices to effect a given change in output, and hence, the greater will be the change in the division of output between *A*, the owner mainly of land, and *B*, the owner mainly of labor.

<sup>3</sup> The model used was originally conceived of by J. Fei, and jointly developed by Fei and the author.

<sup>4</sup> The ratio of land to labor owned by *B* will necessarily fall short of the land-labor ratio of the economy.

(b) The difference in the ratio of land to labor owned by  $A$  and  $B$ . The greater is this difference, the greater will be the change in the division of output between  $A$  and  $B$  that will result from a small change in the pattern of output from  $J_1$ . The position of  $KL$  with respect to the origin depends upon the absolute amounts of land and labor owned, respectively, by  $A$  and by  $B$ . The more of both factors  $A$  owns, the farther out from  $O$  will  $KL$  be. Rather than develop the  $KL$  function rigorously—which is, at best, a tedious operation—we shall let this intuitive argument suffice.<sup>5</sup>

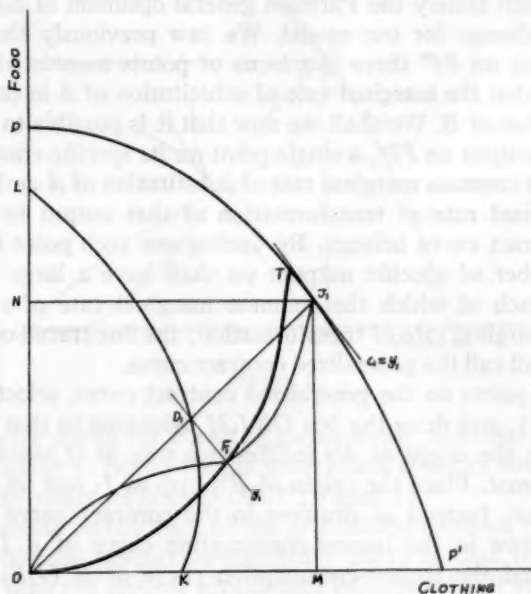


FIGURE 1

3. A specific contract curve ( $OF_1J_1$  in Figure 1). This function is the locus of points for a particular output which satisfy the Paretian general optimum of exchange. For any possible output combination such as  $J_1$  there is a box  $ONJ_1M$  "belonging" to that output. The dimensions of the box give the total amounts of food and clothing available for consumption by both  $A$  and  $B$ . If we place the origin of  $A$ 's indifference map at  $O$  and the origin of  $B$ 's map at  $J_1$ , then it is possible to define the locus of point such that it is not possible to increase the welfare of either  $A$  or  $B$  without reducing the welfare of either one or the other by a redistribution of the given output between the two. The

<sup>9</sup> For a somewhat different discussion of this relation, cf. W. F. Stolper and P. A. Samuelson, "Protection and Real Wages," *Rev. Econ. Stud.*, Nov. 1941, IX, 62-69 (also reprinted as Chapter 15 in *Readings in the Theory of International Trade*, ed. by H. S. Ellis and L. A. Metzler [Philadelphia, 1949], ff. 335-57).

locus is the Edgeworth contract curve. Since each point on  $PP'$  defines a consumption box and a specific contract curve there exists a family of specific contract curves, one for each possible output combination. An important characteristic of specific contract curves is that the slopes (marginal rates of substitution) at which the indifference curves of  $A$ 's system are tangent to those of  $B$ 's system are free to vary throughout the length of the specific contract curve.

4. A generalized contract curve ( $OF_1T$ ). This function is the locus of points which satisfy the Paretian general optimum of both production and exchange for our model. We saw previously that for any specific output on  $PP'$  there is a locus of points associated with that output such that the marginal rate of substitution of  $A$  in consumption is equal to that of  $B$ . We shall see now that it is possible to define, for any specific output on  $PP'$ , a single point on its specific contract curve such that the common marginal rate of substitution of  $A$  and  $B$  is equal to the marginal rate of transformation of that output to which the specific contract curve belongs. By finding one such point for each of a large number of specific outputs we shall have a large number of points for each of which the common marginal rate of substitution equals the marginal rate of transformation; the line traced out by these points we shall call the generalized contract curve.

To locate points on the generalized contract curve, select an output  $J_1$  (Figure 2), and draw the box  $ONJ_1M$  belonging to that output. As before, place the origin of  $A$ 's indifference map at  $O$  and let it range to the northeast. Place the origin of  $B$ 's map at  $J_1$  and let it range to the southwest. Instead of drawing in the contract curve specific to output  $J_1$ , draw in the income-consumption curve of  $A$  ( $OF_1GJ_1$  in Figure 2) and the income-consumption curve of  $B$  ( $J_1F_1HO$ ), both with reference to the price  $y_1$ . Since  $y_1$ , the relative price we take to be constant for the purpose of deriving the income-consumption curve for  $A$  and for  $B$ , is equal to the marginal rate of transformation at  $J_1$ , the income-consumption curve of  $A$  is the locus of points for which the marginal rate of substitution of  $A$  in consumption is equal to the marginal rate of transformation at  $J_1$ . The same is true for the income-consumption curve of  $B$ . At the point of intersection of these two curves ( $F_1$  in Figure 2), the common marginal rate of substitution of  $A$  and  $B$  will be equal to the marginal rate of transformation for output  $J_1$ . Hence,  $F_1$  is a point on the generalized contract curve, and we may say that point  $J_1$  on the production-possibility function "contributes" point  $F_1$  to the generalized contract curve. It may be noted that the contract curve specific to output  $J_1$  (not shown in Figure 2) also necessarily passes through  $F_1$ .

By selecting various other points on  $PP'$  different income-consump-

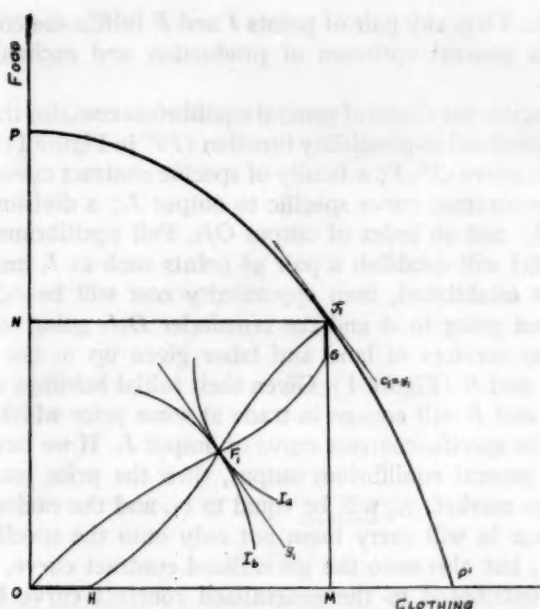


FIGURE 2

tion curves for *A* and for *B* can be constructed; and their intersections will give additional points on the generalized contract curve.<sup>6</sup> By connecting these points we draw in  $OF_1T$  (Figure 1).

The economic significance of the generalized contract curve in conjunction with the production-possibility curve is that if competition does exist, then the general equilibrium established will yield a pair of points, one on the production-possibility curve and the other on the generalized contract curve, such that the marginal rate of transformation (opportunity costs) between food and clothing in the productive sector will be equal to the marginal rate of substitution between food and clothing for both *A* and *B* in the exchange market. This condition is represented in Figure 1 by all possible pairs of points *J* and *F*; that is, a point on the production-possibility curve and the point contributed to the generalized contract curve by this point on the production-pos-

<sup>6</sup> It is possible to select points on the production possibility function which "have" nonintersecting income-consumption curves. Since the forces of competition would never lead the economy to such a point on  $PP'$  we can disregard these points.

On the other hand, it is entirely possible that a pair of income-consumption curves could intersect more than once. Such an *embarras de richesse* would make it very difficult actually to draw in the generalized contract curve, as has been done in Figure 1, but would in no way alter, or detract from the meaning or significance of the generalized contract curve.

sibility curve. Thus any pair of points  $J$  and  $F$  fulfills the conditions of the Paretian general optimum of production and exchange for our model.

To summarize, our model of general equilibrium contains the following elements: a production-possibility function ( $PP'$  in Figure 1); a generalized contract curve  $OF_1T$ ; a family of specific contract curves of which  $OF_1J_1$  is the contract curve specific to output  $J_1$ ; a division-of-output function  $KL$ ; and an index of output  $OJ_1$ . Full equilibrium in a competitive model will establish a pair of points such as  $J_1$  and  $F_1$ . If an output  $J_1$  is established, then opportunity cost will be  $c_1$ , with  $OD_1$  of  $OJ_1$  output going to  $A$  and the remainder  $D_1J_1$  going to  $B$  in payment for the services of land and labor given up to the productive sector by  $A$  and  $B$  (Figure 1). Given their initial holdings of food and clothing,  $A$  and  $B$  will engage in trade at some price which will carry them onto the specific contract curve of output  $J_1$ . If we have, by luck, chosen the general equilibrium output, then the price established in the exchange market,  $y_1$ , will be equal to  $c_1$ , and the exchange that  $A$  and  $B$  engage in will carry them not only onto the specific contract curve at  $F_1$ , but also onto the generalized contract curve, since  $F_1$  is the point contributed to the generalized contract curve by  $J_1$ . Full equilibrium will be established with the marginal rate of transformation ( $c_1$ ) equal to the marginal rate of substitution for both  $A$  and  $B$  ( $y_1$ ).

To facilitate the analysis of excise taxes, we shall make one more assumption with respect to the organization of the economy: The productive sector is assumed to be entirely separate from the exchange market and the only information the productive sector has on which to act, aside from a knowledge of production functions, is the relative prices of goods and services; that is,  $A$  and  $B$ , as exchangers, are assumed not to communicate directly with the productive sector.

## II. Price, Output, and Income Effects

We turn now to the analysis of the effects that an excise tax on one of the two commodities will have on commodity and factor prices, the composition of output, the division of output between  $A$  and  $B$ , and the welfare of  $A$  and  $B$ . The pair of points  $J_1$  and  $F_1$  in Figure 3 characterize a possible competitive equilibrium position of our system in the absence of any excise taxes. Now let an excise tax of  $x$  per cent be applied to the sale of clothing. This means that the purchasers of clothing will be required to pay for it a price of  $(y_1 + t_1)$ , where  $t_1$  is  $x$  per cent of  $y_1$  while the purchasers of food will be able to exchange clothing for food at the old price of  $y_1$  as long as output  $J_1$  persists. The first and lasting effect of this tax is to destroy the efficiency with which output  $J_1$  is

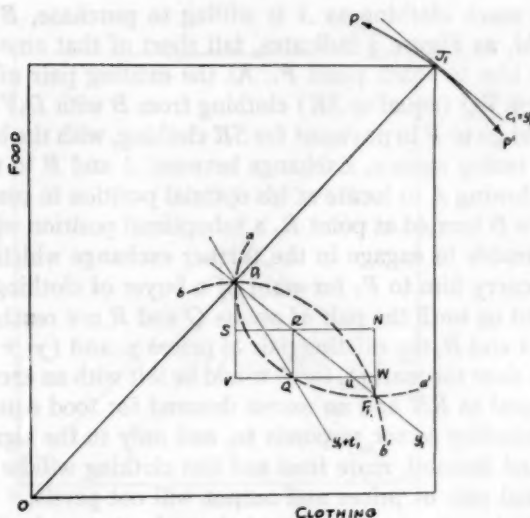


FIGURE 3

distributed by requiring  $A$  to pay a different price for clothing ( $y_1 + t_1$ ) than  $B$  receives ( $y_1$ ) for selling clothing to  $A$ .<sup>7</sup> With different prices facing our two exchangers, it is no longer possible for both  $A$  and  $B$  to equate their marginal rates of substitution between food and clothing to a single price, and hence the marginal rate of substitution between food and clothing will no longer be the same for  $A$  and for  $B$ . A second effect of the tax is to destroy the equilibrium of the system and to require a new general equilibrium to be found which is consistent with the tax on clothing.

At the new price ( $y_1 + t_1$ ) confronting him,  $A$  would want to exchange food for clothing until he again equated his marginal rate of substitution to price. This would occur at point  $Q$ , the intersection of ( $y_1 + t_1$ ) with  $A$ 's offer curve  $aa'$ , which would require  $A$  to exchange  $D_1V$  food for  $VQ$  clothing. On the other hand, since  $B$  is a seller of clothing and a buyer of untaxed food, his plans would not be affected by the tax on clothing. Price  $y_1$  would remain the price at which he could exchange clothing for food, and point  $F_1$  would continue to be the optimal position in consumption for  $B$ . However, since  $B$  can sell

<sup>7</sup> "An excise tax interferes with allocating efficiency, because it makes the relative price of the taxed commodity different for different members of the community—producers being interested in the price net of tax, and consumers in the price including tax," T. Scitovsky, "A Reconsideration of the Theory of Tariffs," *Rev. Econ. Stud.*, Summer 1942, IX, 94 (also reprinted as Chapter 16 in *Readings in the Theory of International Trade*, ed. by H. S. Ellis and L. A. Metzler [Philadelphia, 1949]).

to  $A$  only as much clothing as  $A$  is willing to purchase,  $B$ 's sales of clothing would, as Figure 3 indicates, fall short of that amount necessary to allow him to reach point  $F_1$ . At the existing pair of prices,  $A$  would purchase  $VQ$  (equal to  $SR$ ) clothing from  $B$  with  $D_1V$  food,  $D_1S$  of which would go to  $B$  in payment for  $SR$  clothing, with the balance  $SV$  taken by the taxing agency. Exchange between  $A$  and  $B$  to this extent only, while allowing  $A$  to locate at his optimal position in consumption,  $Q$ , would leave  $B$  located at point  $R$ , a suboptimal position with respect to price  $y_1$ , unable to engage in the further exchange which would be necessary to carry him to  $F_1$  for want of a buyer of clothing. If, then, trade is carried on until the pair of points  $Q$  and  $R$  are reached in consumption by  $A$  and  $B$ , the existing pair of prices  $y_1$  and  $(y_1 + t_1)$  would have failed to clear the market, for  $B$  would be left with an excess supply of clothing equal to  $RN$  and an excess demand for food equal to  $NF_1$ . Since the productive sector responds to, and only to the signals of excess supply and demand, more food and less clothing will be produced, and the original pair of prices and output will not persist.

Adjustments in output will be carried out in the productive sector, which will in turn require changes in relative factor prices, until an output is arrived at in the productive sector and a pair of prices is found in the exchange market which clears the market; that is, a pair of prices which makes it possible for  $A$  to equate his marginal rate of substitution between food and clothing to the price-plus-tax, and  $B$  to equate his marginal rate of substitution to the price established, with excess supply and demand of either commodity equal to zero.<sup>8</sup> In Figure 4, let this pair of equilibrium prices be  $y_2$  and  $(y_2 + t_2)$ , and let the output be  $J_2$ . The price of food would be higher than that shown by  $y_1$  in Figure 3, and the price of clothing including tax would be lower than that shown by  $(y_1 + t_1)$ . The new output,  $J_2$ , would be made up of a higher proportion of food than in the case of  $J_1$ . The division-of-output function would tell us, if it were drawn in, that  $OD_2$  of food and clothing would go to  $A$  and the remainder to  $B$ .  $A$  will exchange  $D_2V$  food for  $VQ$  clothing and locate at point  $Q$  in consumption.  $B$  will exchange  $VQ$  clothing for  $D_2S$  food and locate at point  $N$  in consumption. The remainder of the food,  $SV$ , will go to the taxing agency, and for the present we ignore the possible effects of the spending policy of this agency. Both  $A$  and  $B$  achieve their optimal positions in consumption,

<sup>8</sup> It is not within the scope of this paper to discuss the stability conditions of the model. We shall assume that positions of stable equilibrium do exist. It can be shown that stability depends on the preference functions of all individuals, the asset structure of all individuals (by this we mean the ratio in which individuals hold land and labor and not the absolute amounts held), and the production functions of all commodities. The more similar are the first two among individuals and the last among commodities, the more stable will the system likely be. It appears that similarity breeds stability.



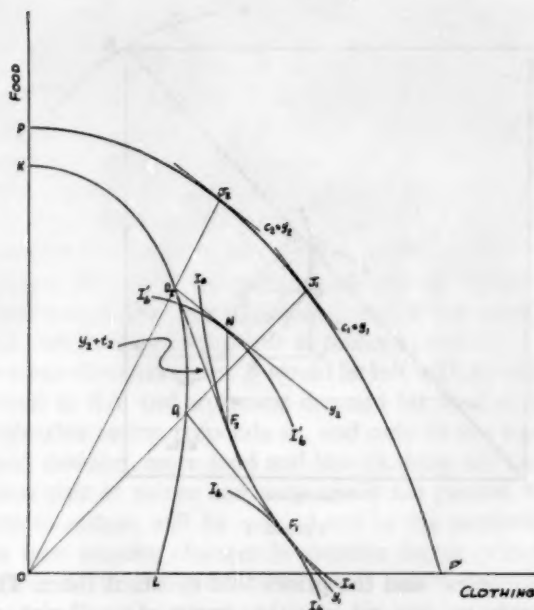


FIGURE 5

At first glance it looks as if the burden of the excise tax falls directly and entirely on *A*, the purchaser of clothing, and not at all on *B*, the purchaser of food. But because *A* "pays the tax" in the crude sense of handing over a certain amount of food to the taxing agency, this does not mean that the entire burden of the tax does fall on *A*, in the sense that *A* becomes worse off after the tax is levied, and that *B*'s welfare is unaffected by the tax on clothing. In fact, it is possible to define situations in which *A* is better off as a result of the tax, and situations in which his welfare is unaffected by the tax, as well as situations in which he is made worse off by the tax.

From the shape of the *KL* function in Figure 5 we see that when clothing is taxed and, as a result, the output of food increases, *A*'s share of output increases. This is because *A* owns a more than proportionate

clothing, but to the productive sector and transform it into *VW* clothing? The answer is that we assume that the productive sector responds to the signals of excess supply and demand only and not to the implorings of individuals. In the economy of the United States it is not possible for individuals to go to Durham, say, and directly transform their labor power into cigarettes to avoid paying federal, state, and local taxes. An excise tax on clothing can be collected only if clothing is purchased in the exchange market, and although *A* may refrain from buying clothing, he is forbidden to transform his excess supply of food into clothing in the productive sector.

share of land, the factor whose relative price increases as the output of food increases. The question now is whether  $A$  will be better or worse off after paying taxes on the clothing he purchases. It is possible to conceive of an after-tax equilibrium such that we would have an output combination  $J_2$  (Figure 5), a pair of prices  $y_2$  and  $(y_2 + t_2)$ , and a division of output  $D_2$  which together would result in  $A$  being just as well off after the tax as before the tax, leaving the full burden of the tax to be borne by  $B$ . In this special case the benefit of an increased price for labor accruing to  $A$  would be just offset by the burden of an increased price for clothing on  $A$ . With output  $J_2$  divided at  $D_2$  and with a price for clothing of  $(y_2 + t_2)$ , where  $y_2$  is less than  $y_1$ , but  $(y_2 + t_2)$  is greater than  $y_1$ ,  $A$  would exchange food for clothing up to  $F_2$ , on indifference curve  $I_a$ , the curve he was originally on at point  $F_1$ .  $B$  would exchange clothing for food at a price  $y_2$  up to point  $N$  on indifference curve  $I'_b$ , which is lower in his preference system than is curve  $I_b$ , the curve he was originally on at point  $F_1$ . Hence, even though  $A$  pays the tax by handing over to the taxing agency  $F_2N$  food as he purchases clothing, the entire burden of the excise tax would be shifted to  $B$  if output were to change in response to the excise tax from  $J_1$  to  $J_2$ .

For shifts of output into food greater than the shift from  $J_1$  to  $J_2$ ,  $A$ 's post-tax position will actually be superior to his pretax position, and  $B$  would suffer not only the entire tax burden, but also the burden of  $A$ 's enhanced income position. For shifts of less than  $J_2$ , the burden of the tax will be divided between  $A$  and  $B$ , with  $A$  bearing the entire burden in the unlikely event that there is no output shift into food. In the even more unlikely case of clothing being an inferior good to  $A$ , a tax on clothing would give rise to a greater output of clothing, and  $A$  would then bear the entire burden of the tax plus the burden of  $B$ 's superior income position.

The magnitude of the output shift into food required to place the entire burden of the tax on  $B$  depends upon the rate at which  $A$ 's share of output increases as more food is produced. This rate, shown by the curvature of  $KL$ , as we have seen<sup>11</sup> depends mainly on the following two factors: (a) The ratio in which  $A$  (and hence  $B$ ) own land and labor. The greater the ratio of land to labor owned by  $A$  the greater will be  $A$ 's increase in the share of output as more food is produced. (b) The difference in factor utilization between the labor-intensive clothing industry and the land-intensive food industry. When factor intensities are different a change in the composition of output will require, assuming full employment, factor-price changes in favor of the factor intensively used by the expanding industry, which in this case, is land. The

<sup>11</sup> *Supra*, p. 346.

greater is this difference in factor intensities, the more the price of land must rise and the price of labor fall, and the more will *A* benefit by a given change in the composition of output in favor of food.

The actual change in the composition of output in favor of food that does take place in any given case depends upon the following four factors:

1. The excise tax levied on clothing. The greater is the excise tax levied on clothing, the greater will be the shift into food.

2. The marginal rate of substitution for *A* and for *B* between food and clothing. The closer substitutes food and clothing are for *A*, the greater will be *A*'s shift into food when clothing is taxed. For *B*, as more food is produced, its price in terms of clothing will increase, and the less close substitutes these two commodities are, the less rapidly will *B* decrease his consumption of food as its price increases.

3. The tax-expenditure policy of the taxing agency. The greater is the proportion of tax receipts "spent" on food, the greater will be the output shift into food. In the above analysis we have implicitly assumed that the taxing agency purchased no clothing from *B* and did purchase, in effect, only food with its tax receipts. Alternatively, the taxing agency could spend its entire tax receipts *SV* (Figure 3) on *QU* clothing by engaging in exchange with *B* at the untaxed price of  $y_1$ . As a result of this additional exchange *B* would find it possible to locate at point *U* in consumption and consequently his excess supply of clothing would be reduced from *RN* to *UW* and his excess demand for food reduced from *NF*<sub>1</sub> to *WF*<sub>1</sub>. The increase in the output of food and the decrease in the output of clothing necessary to wipe out the existing excess supply and demand for clothing and food, to bring the system into general equilibrium, would then be smaller than before. If point *U* were located on top of point *F*<sub>1</sub>, then the equilibrium of the system would not be disturbed by the tax, for both excess supply and demand would remain equal to zero. There would then be no change in the composition of output in response to the tax on clothing and the entire burden of the tax would fall on *A*, the purchaser of taxed clothing. For a more complete analysis of the equilibrium position of the system after an excise tax is applied, knowledge of a "government preference function" or a set of expenditure plans stating the proportions of food to clothing on which the taxing agency would spend its tax receipts at various levels of prices and receipts would be required.

4. The difference in the land-to-labor ratio used in the labor-intensive clothing industry and the land-intensive food industry at given factor prices. The smaller is this difference, then the closer substitutes food and clothing are for each other in production. The closer the two commodities are substitutes for each other in production, the greater will be the

output response of food to a given increase in the price of food. In geometric terms, the closer food and clothing are substitutes for each other in production, the flatter will be the curvature of  $PP'$ , the production possibility curve, in the neighborhood of point  $J_1$  (Figure 3). The more closely  $PP'$  parallels  $y_1$  in the neighborhood of  $J_1$ , then the greater will be the increase in the output of food in response to a given increase in the price of food.

Hence, the manner in which the incidence of the burdens and benefits of an excise tax rests upon  $A$  and  $B$  depends upon the production functions of both commodities, the structure of asset holdings and the preference functions of both individuals, and the government tax and expenditure policy. This is, of course, not a surprising result, and many economists might have long suspected that a careful application of general equilibrium analysis would yield these conclusions as a matter of course.

### III. Conclusions

It has not been possible for the author to construct an analogous model for an  $n$ -dimensional economy. However, on the basis of the preceding analysis, it does appear possible to draw a few rather weak conclusions regarding the burdens and benefits an excise tax will bring about in a more complicated economy.

In the usual case where elasticities of demand are greater than unity, total spending on the output of any given industry will decrease as that industry is taxed. The output of the taxed commodity will decrease and its price will increase. Demand for the complements of the taxed commodity will decrease and both the price and output of these commodities will fall. Resources will be released by the taxed industry and industries producing commodities complementary to the taxed commodity. Increased spending will be directed toward the output of industries producing substitutes for the taxed commodity and to the industries producing commodities complementary to the substitute commodities of the taxed commodity. Additional resources will be demanded by these expanding industries. If the economy is to respond to the change in spending with a change in the composition of output, it will be necessary for relative factor prices to change if it is the case—as *a priori* appears likely—that the contracting industries employ factors, and hence release them, in different proportions from those characteristic of the potentially expanding industries.<sup>12</sup> The relative prices of certain factors will fall, irrespective of whether they are employed by

<sup>12</sup> On this point, see J. Robinson, "Rising Supply Price," *Economica*, N. S., Feb. 1941, VIII, 1-8; reprinted in *Readings in Price Theory*, ed. by G. J. Stigler and K. E. Boulding (Chicago, 1952), pp. 233-41.

the contracting industries or all other industries,<sup>13</sup> and the owners of these factors will be, *ceteris paribus*, worse off because of the tax. The relative prices of certain other factors will rise, no matter where they are employed, and the owners of these factors will be, *ceteris paribus*, benefited because of the tax. In order to know just which factors will be made worse off and which better off, it would be necessary to know: (a) the industries away from which consumers direct their spending, and the industries toward which they direct their spending, as the output of one industry is taxed; (b) the direction of spending of the additional tax receipts by the taxing agency; and (c) the proportions in which the expanding industries and the contracting industries employ the various factors of production. The excise tax will also exert a burden on the consumers of the taxed commodity, the substitutes of the taxed commodity, and the complements of these substitutes; and the burden will be heaviest for those consumers for whom there exist few, or no close substitutes for the taxed commodity. The excise tax will benefit not only those owners of factors of which the prices have increased, but also the consumers of the complements of the taxed commodity.

Again, if we knew the preference functions and asset holdings of all individuals, the production functions of all commodities, and the tax and expenditure policy of the taxing agency, we could arrange all individuals on a "benefit-burden" scale according to how their welfare is affected by an excise tax on a particular commodity or group of commodities. The burden limit of this scale would be occupied by those individuals whose welfare would be reduced the most by the effects of the tax: those individuals who are heavy consumers of the taxed commodity, its substitutes, and the complements of its substitutes—that is, consumers who happen to have strong preferences for those commodities whose prices increase because of the tax—and who are also owners of factors which are intensively used by the contracting industries and which suffer a relative price decrease. The benefit limit of the benefit-burden scale would be occupied by those individuals whose welfare would be increased the most by the effects of the excise tax: those individuals who have strong preferences for the complements of the taxed commodity and for whom there exist many close substitutes for the taxed commodity, and who are also owners of factors intensively used by the expanding industries. The remaining individuals fall somewhere between these limits, depending on how strongly the benefit-burden forces of increased—or decreased—commodity and factor prices operate on each individual as a consumer and as a factor-owner.

Unfortunately, it appears that for the  $n$ -dimensional case, just as for

<sup>13</sup> Since factors of production are assumed to be mobile.

the two-dimensional case, more knowledge is necessary than is available to place individuals on such a benefit-burden scale. Even worse, one crude inference which it was possible to make in the two-dimensional case cannot be made for the  $n$ -dimensional case. We know that with only a labor-intensive clothing industry and a land-intensive food industry, a tax on clothing will, *ceteris paribus*, make owners of land better off because the price of land would increase relative to the price of labor. However, in the  $n$ -dimensional case, it is not possible to know beforehand, in the absence of complete information, which factors will benefit from a shift in demand, simply because it will not be known to which industries spending will be transferred.

Our knowledge of incidence for any "real" economy now appears to very slight indeed. We do know, however, that excise taxes exert benefits as well as burdens on individuals; and we are also aware of the main factors involved in the spread of the benefits and burdens of an excise tax in a competitive economy. The foregoing observations further suggest that more specific results concerning the incidence of excise taxes can be obtained for those cases in which additional appropriate assumptions regarding the shapes of the relevant parameters (preference functions, production functions, relative size of industry taxed, etc.) can be made in such a way as to eliminate the general equilibrium consequences of these taxes and confine their effects to definite individuals, or groups of individuals, in the economy. For example, the long-established theorem of partial equilibrium analysis that the burden of an excise tax on a particular commodity will be divided among the consumers and producers of the taxed commodity according to the elasticities of supply and demand requires only the additional assumption that the taxed industry is so small that the prices of all *other* commodities and the prices of *all* factors do not change.<sup>14</sup>

<sup>14</sup> For statements of this theorem, cf. Edgeworth, *op. cit.*, pp. 48-53; Marshall, *op. cit.*, pp. 413-15; J. F. Due, *The Theory of Incidence of Sales Taxation* (New York, 1942), pp. 17-53. For a somewhat different example of the complementarity between the more established types of excise tax analysis and the analysis presented in this paper, cf. J. A. Stockfish, *op. cit.*

## POST-KEYNESIAN ECONOMICS<sup>1</sup>

### *A Review Article*

By ALVIN H. HANSEN\*

This is a collection of essays by fifteen authors whose names are for the most part well known. The book is divided into three sections, the first of which relates to monetary theory and policy; the second, to economic fluctuations and growth; and the third, to aggregative economics and testing. On a somewhat different basis of classification, three chapters are empirical in character, two compare current economic thinking with earlier doctrines (Marx and the classicals), five are concerned with monetary theory and policy, four with cycle policy (investment control, depreciation policy, income distribution, institutional change), and one with nonlinear cycle theories. While Part III is much the best, some of the chapters in the first two parts make rewarding reading. And though my review is essentially critical, I feel that the book is decidedly worth while.

Does the volume live up to its title *Post-Keynesian Economics*? Yes and no. It is post-Keynesian in the sense that most of the discussion is cast in terms of the Keynesian tools of analysis. It is post-Keynesian in that the endeavor is made here and there to improve on Keynes, but the result in this respect is far from impressive. It is post-Keynesian in the sense that a part of the volume is devoted to "filling the empty boxes" of the Keynesian analysis with empirical data. Especially notable here is Klein's chapter—an outstanding contribution—and those by Tarshis and Modigliani. It is post-Keynesian in the respect that it seeks in two chapters (Tsuru and Streeten) to assess the Keynesian stream of thinking against the background of earlier traditions. It is post-Keynesian in the respect that it takes a fresh look in a number of chapters at policy programs stemming from the Keynesian system.<sup>2</sup>

On balance the gleanings must be set down as relatively meager, though the volume does contain, as indicated above, a number of notable chapters. That the gleanings are somewhat meager both with respect to tools of analysis and policy matters, after nearly twenty years of voluminous discussion, makes Keynes' individual work stand out all the more strikingly as a truly Herculean contribution to modern economics.

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<sup>1</sup>K. K. Kurihara, editor, *Post-Keynesian Economics* (New Brunswick: Rutgers Univ. Press. 1954. Pp. xviii, 442. \$8.50).

<sup>2</sup>The title *Post-Keynesian Economics* might, however, lead the reader to think that the volume perhaps attempts a survey of the literature which has appeared since 1936. This is not the case. Current literature is of course touched upon here and there but only in a casual manner.

First let us consider the empirical studies. Keynes' system is built on the foundation of several specific hypotheses concerning human behavior. Does empirical investigation support or contradict this foundation? This is the question to which Klein addresses himself. He notes that the essence of Keynesian economics can be stated as follows: "The system of classical competitive equilibrium does not automatically lead to a stable solution of full employment" (p. 281).

The only empirical findings to date, says Klein, that are in any way contrary to the essence of Keynesian economics are the data on the influence of liquid wealth on saving—the so-called Pigou effect. But the empirical evidence is not clear-cut against the Keynesian theory, the evidence being mixed in direction of effect. At all events, the magnitude of the "marginal asset effect on saving is probably not large enough to render market forces adequate operators of adjustment towards full employment equilibrium" (p. 294).<sup>3</sup>

Klein's chapter, which no economist can afford to neglect, is remarkable for the vast range of empirical materials, covering all aspects of the Keynesian analysis, which are surveyed. He concludes that the Keynesian system is "firmly rooted in fact" and that any reader, whether convinced or not, must at any rate agree that the empirical evidence is not superficial or casual.

Tarshis has given us, in his essay on "The Flow of Business Funds, Consumption and Investment," a welcome contribution. Keynes put forward the proposition that consumption is a function of *national* income. Much of the literature, however, has restricted itself to the relation of consumption to disposable income—a much narrower concept. When consideration is given to the multiplier effect of an increase in investment or a reduction in the taxes on consumers, it is the marginal propensity to consume out of an increment of *national* income that is relevant. The bites taken by the progressive rise in business saving and by a progressive tax system (individual and corporate combined) are far more important than the saving of individuals in determining the slope of the consumption function and so the multiplier. We have had too little empirical investigation of the "tax bite" and of the "business savings bite." Keynes, as Tarshis reminds us, had much to say about the important role of dividend policy and depreciation policy in establishing the relation between income and consumption.

The gross national product is equal to consumption plus individual saving plus gross business saving plus *net* taxes. By "net taxes" I mean total tax receipts minus transfer payments. Tarshis calls this "government saving," but this seems to me an unfortunate term. He presents tables for these three categories from 1929 to the present, showing their relative importance in holding consumption below the GNP. Personal saving throughout the period has played a relatively small role.

<sup>3</sup> Referring to two recent studies of the consumption function (Ruth Mack's in *A Survey of Contemporary Economics*, Vol. II [Homewood, Ill., 1952], and R. Ferber's *A Study of Aggregate Consumption Functions*, Nat. Bur. Econ. Research Tech. paper [New York, 1953]), Klein concludes that neither of these studies is sufficiently discriminating in its choice of empirical studies since they "draw upon results that are open to serious criticism from an econometric point of view" (pp. 294-95).

Tarshis also examines the impact of business saving on investment (pp. 381-86) and the role of *sources* of funds (retained earnings, stock issues, borrowing) on decisions to invest. Keynes undoubtedly did not pay adequate attention to the availability of internal corporate funds and their impact on investment decisions.

The third empirical essay is the chapter on "Utility Analysis and the Consumption Function" by Modigliani and Brumberg. What is the impact of long-range plans and expectations on consumption expenditures? Current income, expected future income, initial assets, and age, are among the more significant factors. Since it is not possible to obtain the relevant data from the observation of individuals over time, the authors content themselves with "cross-section" data on the average and marginal rates of consumption with respect to household incomes.

From their so-called "stationary" cross-section data, they conclude that (despite appearances to the contrary) the *proportion* of income saved is in reality independent of the household income bracket, but tends to rise with age. The regression of consumption on income *tends* to be linear and a line fitted to the data tends to go through the origin. But a household whose income *unexpectedly* rises will save more than the normal ratio.

These propositions are certainly rather novel. Margaret Reid had, however, already suggested that there is "good reason to believe the percentage of income saved to be independent of the economic level of the separate families."<sup>4</sup> Earlier writers (Brady and Friedman) have found from family budget studies that the *ratio* of saving to income is a function of the family's position on the Lorenz income distribution curve—a proposition contrary to the one stated above. Nevertheless the ratio of aggregate saving to aggregate family income *over time* is not thought to be a function of income.<sup>5</sup> This however does not tell us what is the relation of consumption to *national* income either in the business cycle short run<sup>6</sup> or over the longer run.

Now Modigliani and Brumberg agree indeed that the data do show that the *ratio* of saving to income is a function of the income bracket in which a household's income falls, but they hold that the higher *ratio* of saving to income in the upper brackets is not due to the fact that they are upper-bracket incomes. Rather, this rising ratio is due to the fact that households in these upper brackets are, by and large, "recent arrivals." Their current incomes are above their *accustomed* incomes, and therefore they tend to save a higher ratio of income. If the new income is regarded as *transitory* the saving ratio will rise. Or, in the event that the new income is regarded as permanent, the initial asset holdings are now out of line with the revised outlook. Thus the saving ratio must rise in order to bring the asset holdings up

<sup>4</sup> See *Savings in the Modern Economy*, ed. by Heller et al. (Minneapolis, 1953), p. 219.

<sup>5</sup> See also James Duesenberry, *Income, Saving, and the Theory of Consumer Behavior* (Cambridge, Mass., 1949).

<sup>6</sup> Modigliani and Brumberg's chapter relates to budget studies and not to the behavior of consumption in relation to national income over the cycle. Thus their attack (p. 43) on those who have employed the Keynesian consumption function is not valid. The secular aspect of the matter is something else again. See in this connection my *Guide to Keynes*

to a level commensurate with the new income.<sup>7</sup> But if all incomes were at their accustomed levels, the ratio of saving to income would, they believe, be quite unaffected by the level of the household's income. In the absence of unexpectedly large incomes, the authors suggest that households save primarily to cushion against major variations in income over the life cycle, and that the savings which a household wishes to make and can afford to make must be basically *proportional* to its basic earning capacity.

This conclusion is admittedly a tentative one and the authors wisely refrain from dogmatism. Whether right or wrong, is the hypothesis at variance with Keynes' formulations with respect to the relation of consumption to income? In answer to this question the authors are, to put it mildly, a bit irresponsible. They fail to point out that their conclusion does not in the least contradict Keynes' statement that we can take it as a "fundamental psychological rule of any modern community" that, when its real income is increased, it will not increase its consumption by an equal *absolute* amount" (*General Theory*, p. 97). Instead they claim that they depart from Keynes. But their citation from Keynes consists of a casual side remark and not from his central argument. Moreover the citation significantly omits the phrase "as a rule" which, if included, would have shown that Keynes made no firm "contention" about the *proportion* of income saved as income rises.<sup>8</sup> Finally, and this is highly important, Keynes was talking about the behavior of the "modern community" as a whole, including business saving as well as individual saving. Thus Modigliani and Brumberg are not even talking about the same thing as Keynes! And even though they were, it is not true that their findings (if verified) would show Keynes to be in error.

Tsuru is concerned with the history of economic ideas with special reference to Keynes and Marx. The appearance of the *General Theory* caused at first a sharp division among the ranks of economists between those who emphasize the income effect and those who emphasize the "efficacy of cost-price relation-

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(New York, 1953), pp. 75-78; also my *Business Cycles and National Income* (New York, 1951), pp. 164-70.

Moreover, the proposition that the *ratio* of saving to income is independent of income is often misinterpreted to mean that *saving* is not a function of income. This misconception has led to a vast amount of confusion, and to quite unfounded charges that Keynes' position has been proven to be in error.

<sup>7</sup>This lag effect is emphasized by Keynes on p. 97 of the *General Theory* where he discusses the difference between one's "actual income" and one's "habitual standard." But is this sufficient to explain the higher saving ratio in the upper income brackets? Is it not also true, as Keynes put it (p. 97), that the "motives towards accumulation" only acquire effective sway "when the margin of comfort has been attained"?

<sup>8</sup>Note that Keynes speaks of the modern *community*, and that his elaborate discussion of the psychological behavior pattern of the community involves not only the behavior of individuals as consumers, but also as government officials managing sinking-funds, and business officials managing depreciation funds, etc. The current literature is full of misinterpretations of Keynes' "psychological law."

<sup>9</sup>Indeed in the very next sentence, following the phrase which our authors quote, Keynes explicitly disavows any firm contention, for he says: "But whether or not a greater proportion is saved, we take it as a fundamental psychological rule . . . etc." (The completed sentence is given above.)

ships." This division has gradually given way to "an attempt at synthesis." Marx, while recognizing the effectiveness of price adjustments, developed his theory largely in terms of "long-run normal price." "In other words, 'parametric adjustments' have no place in the stage of abstraction where Marx took up his aggregative analysis" (p. 333). Tsuru attempts to contrast the Keynesian aggregates and the Marxian aggregates in economic analysis. This effort necessarily requires a good deal of reading between the lines.

Streeten's excellent chapter is on "Keynes and the Classical Tradition." Attention is centered around the "harmony of interest" doctrine. "Keynesian theory strengthened the utilitarian tradition because it resolved one of the great moral dilemmas of the neo-utilitarians" (p. 351). A more egalitarian income distribution might raise general welfare, but inequality appeared to be necessary to safeguard adequate savings and so investment. Thus greater equality might defeat, as the utilitarians saw it, its own purpose. But Keynes' consumption function analysis showed that a decrease in thrift might in fact raise income and investment, and so there might well be no conflict.

While Keynes believed in the virtues of the free market and the pricing system, Streeten continues, his measures for regulating investment represent a break with liberal-utilitarian tradition. Moreover, he did not believe, as did Mill, that production would "look after itself." Here he was more like List than the classicals. Keynes thought that the state must take certain actions "in order to create the right environment for private self-interest to work."

Keynesian policies are designed to promote employment and prosperity—the "interests of all." Then why is there opposition to Keynesian policies? There are various reasons, *e.g.*, they may undermine the discipline of workers, entrepreneurs may fear that their power and status in society will be reduced, etc.

Streeten suggests that Keynes believed more strongly in a "harmony of interests" than recent full-employment experience would warrant. Here he calls attention to the oft expressed fear that full employment and price stability are incompatible. (Streeten, however, fails to canvass adequately American experience since 1948.) Reference is also made to the conflict that may arise between full-employment and balance-of-payments problems.

Under large-scale unemployment, one group's gain need not spell another's loss. But with full employment, conflicts over the "full-employment pie" emerge.

In the Victorian society, convention and superstition tended to prevent an open conflict of interest. The gold standard, the balanced budget, the acceptance of unemployment, etc., testify to the "submission to external unquestioned rules and conventions" (p. 362). But this quasiharmony broke down because the loss of these ancient beliefs "stripped the structure of economic relations of the superstitions which had cemented them." Keynesian economics helped to destroy the "barriers which prevented the full pursuit of a selfish manipulation of society," and thus brought "clashes into the open" (p. 362).

Yet Keynes' thought is "unmistakably in the classical liberal-utilitarian

tradition." He advocated the "regulation of aggregates only." Nevertheless, in a country pledged to full employment, the classical conflict of interests has again more or less come into its own, and in addition new problems have arisen.

A number of chapters deal with the business cycle, partly in terms of theory but more often in terms of policy. Matthews' paper is on "Capital Stock Adjustment Theories" and the problem of stabilizing investment at a level "such as not to cause the stock of capital to increase so rapidly that a decline in its marginal efficiency occurs" (p. 173). The boom level of investment is not maintainable for reasons made plain by the capital stock adjustment theories. "The dilemma is, then, that a rate of investment high enough to give full employment leads to excessive capital accumulation and is not maintainable" (p. 173). Investment must therefore be stabilized at lower than the boom level and this would require for full employment a rise in the consumption function; or alternatively there might be offsetting increases in government spending.

Matthews poses this problem in relation to three theories of the cycle: (1) Schumpeter's innovatory investment theory, (2) the Wicksell-Frisch theory of damped cycles sustained by erratic shocks, and (3) the theory of antidamped cycles whose explosive tendencies are contained by various restraints (Hicks, Goodwin). It is the "ceiling theory" to which Matthews devotes primary attention. The ceiling may be imposed by a general shortage of factors or by bottlenecks in the investment industries—the "investment ceiling." In place of the ceiling hypothesis, we have Keynes' view that full employment of factors is likely to "transform a boom into a cumulative inflation of wages and prices" (p. 180). This Matthews calls "Wicksellian instability."

Matthews closes his theoretical analysis with the conclusion that if the Hicks-Goodwin theory can be at all accepted as realistic (a point left unsettled) then cyclical instability must probably be faced and in addition there is the danger of "Wicksellian instability." Thus the authorities are confronted with the policy alternatives already referred to above. Of these alternatives, Matthews favors a policy of direct control of investment. This policy, he believes, is likely to be effective in a condition of overexpansion such as most countries have had to contend with during the last ten years owing to war-created shortages, and more recently to rearmament expenditures. "But we must not shirk contemplation of the time when this will not be so" (p. 191). Unfortunately, however, the essay throws no new light on how we should solve this problem if and when more normal conditions arrive. Thus this very useful essay is more interesting in its theoretical implications than as a guide to policy.

Ichimura's chapter, largely mathematical, is devoted to a survey and comparison of recent cycle theories. He disregards exogenous theories of the Schumpeter or Spiethoff variations, and rules out endogenous linear theories as unsatisfactory, though he grants that when combined with erratic or quasi-oscillatory shocks, such systems have much to commend them. He devotes his chapter, however, to the nonlinear theory of Kaldor and those of the

Hicks-Goodwin type. He begins his essay with an analysis and reformulation of the Kaldor model, and then moves on to a comparison with other recent theories, at the same time throwing light here and there upon the relation of these to earlier theories.

The essay by Bowen and Meier is instructive and challenging. The last twenty years teach us, they think, that "painful changes in deeply-rooted institutions are involved in any effort to achieve stability via fiscal policy" (p. 164). They conclude that "stabilization necessarily involves change in firmly established institutions and is therefore likely to be difficult of attainment" (p. 168). The most difficult obstacles are: (1) the institution of budget-balancing, (2) the ponderous legislation procedures with respect to fiscal policy, (3) intergovernmental relations in a federal system, (4) the traditionally established dichotomy between state and private industry, and (5) the effort of trade unions to raise money wages.

These obstacles form well-established and persistent institutional patterns. It is a delusion, the authors assert, to suppose "that fiscal policy does not require major institutional changes" (p. 165). Fiscal policy of a magnitude to overcome a deep depression "would even today be confronted with strong institutional resistance" (p. 166).

To facilitate a stabilization program they suggest (among other things) a closer coordination of investment and saving involving control over saving or investment, or both. This might involve direct investment by the government and greater equality in income distribution.

The authors agree that a substantial residue of institutional changes bearing on stabilization was left by the social crisis created by the Great Depression—social security, financial reform, a more progressive tax structure, etc. And they agree that the resistance to change may have been "softened up a little by the arguments of economists" (p. 168). But they fear that institutional barriers may before long be "back at par."

Kurihara's chapter is basically a policy chapter, and the program he proposes, to secure full employment and growth, is primarily to raise the consumption function through a redistribution of income. The main defect in the chapter, as I see it, is that his policy directive takes little or no cognizance of social priorities. It may well be that in the visible future what is most urgently needed is public investment expenditures, not only on resource development, roads, etc., but also outlays on health, education, recreation, control of juvenile delinquency, etc., instead of more private consumption via redistribution of income. Greater equality of income up to a point is no doubt desirable, but there may be danger in pushing it so far that the strength of a vigorous middle class—upon which a dynamic society must heavily lean—may be sapped. Over against income redistribution, public investment and deficit financing are important alternative fiscal routes to full employment. The appropriate combination of all possible alternatives requires a careful assessment of social priorities. It is on this basis that we must decide how large the government budget should be, whether it is to be financed by borrowing or by taxes, and if by taxes, what degree of progressivity should be imposed. These matters are somewhat neglected by Kiri-hara

who concentrates his analysis on (1) raising consumption, and thereby (via the acceleration principle) stimulating private investment, and (2) lowering the rate of interest so as to push private investment toward the point of zero marginal efficiency.

Murad's chapter on "Net Investment and Industrial Progress" presents the thoroughly unorthodox view (with respect not only to classical but also to Keynesian theory) that industrial progress *necessarily* tends toward zero net investment. He begins with the well-accepted thesis that increasing capital accumulation, in the absence of growth of population and changes in technique, will reduce the marginal efficiency of capital eventually to zero. Later he introduces technical change, but argues that if technological advances occur at the same rate in producers' goods industries as in consumers' goods industries, no new opportunities for *net* investment will result from such advances.

Appraisal of Murad's argument necessarily involves consideration of what constitutes true replacement and what constitutes true net investment. To this there is no easy answer. Depreciation funds are not used, in a technologically progressive society, to replace the same old-type industrial machines. They are used to buy new and better machines. If now the depreciation sums set aside for replacement are just sufficient to buy new machines capable of producing the *same* output as the old, and if sums so spent accurately measure *replacement* investment, then (contrary to Murad's view) progress will not rule out net investment. Output all around will have increased, let us say, by 100 per cent; money wages (and other money incomes) will also have increased by 100 per cent while prices of both producers' goods and consumers' goods have remained constant. It now requires only half as many laborers to produce the new machines needed to replace the productive capacity of the old machines. Since wages have doubled, the money cost of these new machines is, however, the same as the cost of the old; therefore, if depreciation has been calculated on a money-cost basis, the depreciation sums set aside will be just sufficient to replace the old machines with machines of equal productive capacity. But half of the labor force has been set free. Hence, concomitant with the advance in technology, *net* investment is continuously necessary to provide the displaced laborers with capital equipment and to implement in terms of increasing output, the fruits of technical advance. Thus on the conditions here stated—and they do not violate his assumptions—Murad's proposition is in error.

Murad himself inadvertently admits this point without seeing how damaging it is to his thesis. He notes (footnote, p. 246) Domar's definition of depreciation as the "cost of replacement of the depreciated asset by another one of *equal* productive capacity." Murad objects to this definition on two grounds: (1) that it is not in accordance with accounting practice, and (2) that such a definition would equate all additions to capital (measured in terms of productive capacity) with net investment. The answer to these objections is first, that accounting practice (depreciation calculated on a money-cost basis) would in fact replace equal productive capacity on the assumptions which I have made above; and, second, that Domar's depreciation

does provide the best answer to the difficult question—what indeed is meant by *net* investment.

Now it was already recognized by J. S. Mill (and I have myself frequently stressed this point<sup>10</sup>) that a society which already has a huge stock of capital *may* enjoy increasing productivity without net investment. This conclusion rests, however, on grounds other than those laid down by Murad. It rests basically on two assumptions—assumptions which are probable, or at least possible, but which are not necessarily concomitant with a large accumulated capital stock. These assumptions are primarily: (1) that, in a modern advanced society, technology in fact has tended to produce capital-saving machines<sup>11</sup> (*i.e.*, technical progress in the producers' goods industries tends to exceed that in consumers' goods industries—*e.g.*, services, etc.) and (2) that accounting practice in fact tends to supply depreciation funds in excess of the amount required to maintain the same productive capacity.<sup>12</sup>

Finally we come to chapters dealing with monetary theory and monetary policy. I am not at all happy about any of these chapters, and I fear that my comments may be overly critical. Dillard's chapter on "The Theory of a Monetary Economy" begins with high promise but fades away at the end, it seems to me, into mysticism. Dillard tries to make something quite special out of Keynes' "Essential Properties of Interest and Money" but fails, I think, to add anything to Lerner's important article.<sup>13</sup> While much of the chapter is well written and thought-provoking, its central thesis does not, in my opinion, stand up well.<sup>14</sup> He attempts to show that in Keynes' theory

<sup>10</sup> In my *Fiscal Policy and Business Cycles* (New York, 1941), I canvassed this matter with some care (see pp. 310-11). Among other things, I stated: "A mature economy may, as Mill stated, under certain conditions modernize and improve its capital equipment, introducing continually new techniques, without tapping any new savings or making any net addition to capital formation. If the progress of technique in the capital goods industries outruns the rise in wage rates, then the accumulated depreciation reserves will be adequate to finance the replacement of an old machine by a new one more productive than the old." Also the following: "When capital saving innovations are made . . . the expenditure of replacement allowances will yield a net increase in productivity."

<sup>11</sup> Murad indeed discusses capital-saving inventions (p. 244), but he does not succeed in clarifying the issue. The definitions which he presents on page 245 are vague and confusing.

<sup>12</sup> See in this connection my "Growth or Stagnation in the American Economy," *Rev. Econ. Stat.*, Nov. 1954, XXXVI, 409-14.

<sup>13</sup> See also my chapter on "Nature and Properties of Capital, Interest, and Money" in my *Guide to Keynes*, *op. cit.*

<sup>14</sup> Dillard attempts to elevate the highly perfunctory chapter which Keynes wrote for the Spiethoff *Festschrift* (Munich, 1933) into a monumental contribution. He asserts that this chapter appears to be unknown to English-speaking economists, probably because it appeared in a German volume. But Keynes' chapter is in English (other English chapters are by Robertson, Mitchell, Carl Snyder, and Hawtrey), and the volume has frequently been cited by English and American writers on business cycles. In my own case, I have used the volume a good deal and have been familiar with the Keynes chapter from the time it appeared. But I have always regarded it, and still do, as a slight piece, probably dashed off in an hour or two and neither a credit to Keynes nor to Spiethoff in whose honor it was written. As one rereads it, it becomes quite clear that all sorts of things which later were spelled out clearly in the *General Theory* (not simply a *monetary* theory) were floating around in his mind. About the only interesting thing in the essay is the announcement (1933) that he was working on an important new book.

"money holds the key to explaining unemployment" (p. 20); again that "the ultimate theoretical explanation of unemployment must be sought in money" (p. 22); or again, that money is "the strategic factor" upon which Keynes' entire analysis focuses (pp. 19-20). This of course is not the first attempt to show that the whole Keynesian system rests on a single pivot.

Dillard is in fact less one-sided than some of his more extreme statements would indicate. The chapter, however, ends somewhat disappointingly as one encounters repetitions of such phrases as "the monetary theory of employment," "the nature of money as the key institution of modern capitalism," money's "role as a special form of property," etc.

Martin Bronfenbrenner believes that events thus far indicate that, for causes quite apart from war and rearmament,<sup>18</sup> we are now in a new phase of secular inflation owing to "pressure economics" and "Keynesian economics" (p. 39). These new developments certainly raise important issues which deserve careful study. But there is some danger in exaggerating current inflationary forces compared with those of earlier periods. Actually, the first quarter of our century was considerably more inflationary than the second quarter.<sup>19</sup> Or again, wholesale and consumer prices combined (prior to 1913, wholesale prices) rose 143 per cent from 1894-97 to 1923-25, but only 100 per cent from 1931-34 to 1948-50. From 1948 to 1955 wholesale prices have risen less than 1 per cent per annum, whereas from 1897 to 1910 they rose nearly 4 per cent per annum. Where were "pressure" and "Keynesian economics" then? Do these figures show as Bronfenbrenner asserts, that the strongest, most aggressive, and best-disciplined of the pressure groups were held in check, in the pre-Keynesian days, by "limitations of demand" (p. 40)?

Bronfenbrenner suggests that the inflationary movement did not subside during the relative peace years preceding Korea (p. 38). But the facts are otherwise. At the outbreak of Korea, prices stood at 157 compared with 165 for 1948. It is not true that "no substantial reversal of the inflationary trend" occurred in 1951 (p. 39). Wholesale prices turned down in February 1951, falling fairly steadily from 114.8 (new index) to 109.5 at the end of 1954.

It thus appears that it is perhaps too early to get overly alarmed about pressure groups and Keynesian economics. Indeed it may be altogether possible (though I would not venture an opinion) that collective bargaining (stable contracts over a considerable period, etc.) and the increasing spread of Keynesian economics—even involving members of Congress (witness the growing awareness of the impact of tax changes on inflation and deflation)—may give us greater price stability than was achieved over considerable periods in the "good old days."

With respect to the future, however, Bronfenbrenner has no illusions that the old methods could any longer be effective to hold down inflation. Labor

<sup>18</sup> Bronfenbrenner asserts that it is a "convenient escape . . . to ascribe the inflation completely to the military factors of war and rearmament" (p. 38).

<sup>19</sup> From 1900 to 1925, wholesale prices and consumer prices (wholesale prices alone prior to 1913) rose by 103 per cent; from 1925 to 1950 these price indexes rose by only 40 per cent.

will no longer tolerate either mass unemployment or gross inequality in income distribution—both powerful deflationary forces. He pictures labor as currently trying the inflation route to full employment and greater equality. But labor will, he thinks, eventually realize the futility of inflation. The drive toward greater equality will however continue and this may tend to dampen growth and efficiency.

Perhaps, but no such tendencies are currently visible. Bronfenbrenner does not adequately appraise the contribution to efficiency and stability of greatly improved labor-management relations—a development which has taken place during the last twenty years of so-called “pressure economics” and “Keynesian economics.”

Turning to Mabel Timlin's paper on “Monetary Stabilization Policies and Keynesian Theory,” the reader will note her failure to assess the *real* factors. This is all the more remarkable since her chapter begins with a reference to Section VI of Chapter 21 in the *General Theory* which stresses the complexity of causes underlying price movements, and especially the real factors.

Miss Timlin is quite prepared to argue that during the immediate postwar years (p. 65) the rate of investment should have been restricted despite the vast backlog of urgent needs for additional plant, equipment, and housing. But would a drastic monetary restraint on investment at that time have been desirable? Dennis Robertson years ago reminded us that there are times when price stability is not necessarily to be preferred to other goals. These other goals may not be wholly realizable if price stability is rigidly pursued. Miss Timlin's paper does not realistically examine the painful choices that confronted Canada, no less than the United States, in the years that followed a *total* war in which nearly half of the nation's resources had for years been devoted to military pursuits. Could the interest-rate policy advocated by Miss Timlin have prevented price-level increases without any sacrifice of output and employment goals? And we should certainly have expected from a Canadian a full discussion of the consequences of a Canadian price policy completely at variance with American developments.

The Canadian policy-makers are roundly criticized for their expectation that the postwar inflationary situation might prove to be temporary, and that later on the problem might well be one of inadequate demand in relation to production capacity (pp. 62, 64). But what is the evidence (assuming a peaceful world) that they were not right? The inflation ran out by 1948, and since then a falling price trend has been interrupted only by the eight months of panic buying following the outbreak of war in Korea. They could scarcely be expected to know that we were soon to move into a costly cold-war situation.

Miss Timlin stresses “adequate control over the quantity of currency and bank deposits, exercised through flexibility of yields on the securities entering into the portfolios of central banks” (p. 86). She is unhappy over the reliance placed by the Canadian government on (a) its budgetary surpluses,<sup>17</sup> (b)

<sup>17</sup> Actually the cash budgetary surplus was enormous in fiscal 1947, and substantial in 1948-50. Miss Timlin surely underestimates their role in checking the 1946-47 inflation.

"suggestions" to the chartered banks, (c) consumer credit regulations, and (d) deferred depreciation tax allowances. She would like to have jumped the yields in a "*sharp and sudden and once-for-all rise*" (p. 64) but she thinks that a lagging and niggardly rise in rates, taking place by small degrees (the policy advocated by the New York Federal Reserve Bank), might aggravate the problem. She does not explain how anyone would know exactly how much that sudden sharp rise should be, or what might be the consequences of a drastic fall in capital values generally.

The *minimum* objective, she thinks, should be to prevent any increase of currency and bank deposits and to deter any flow of securities to the central bank. Actually both of these minimum objectives were achieved in the United States, but this did not prevent the price rise in 1946-47. (Currency and demand deposits stood at \$106 billion in June, 1946, and at \$108 billion in June, 1948,<sup>18</sup> while U. S. securities holdings by the Federal Reserve Banks were \$23.8 billion in June, 1946, and \$21.4 billion in June, 1948.)

Unlike Miss Timlin who assumes price stability as a *summum bonum*, Vickrey poses the question whether "an economy in which prices are rising steadily" may not be more stable than one with a stable price index (p. 89). He posits a "condition of specified, controlled, and generally anticipated inflation as a respectable and possibly even desirable condition" (p. 90).

The key condition, he thinks, is that inflation be generally anticipated. And it is not difficult, he says, to construct models in which *anticipated* inflation does not of itself produce instability. "Models," perhaps, yes. But what of the actual world?

Varying combinations of interest rates, tax rates, and government spending rates may of course be employed to achieve (in model building at least) any desired trend of prices. On balance, Vickrey prefers a high rather than a low money rate since he prefers to operate on a liquidity preference curve that is more nearly vertical.

Vickrey guides the reader through several models, but for the most part he is aware that they have little significance for the real world, however interesting the speculations may be. Among the unrealistic assumptions introduced are the following: (1) real aspects of the economy are unaffected by monetary vagaries and are also fully anticipated; (2) public confidence in the maintenance of a precisely *steady* rate of inflation, say 10 per cent per year<sup>19</sup> (pp. 110, 112, 118).

Currently "wartime destruction and the demands of rearmament" have pushed the productivity of capital up into a range which gives, he thinks, sufficient margin for the effective operation of monetary controls (p. 122). But he concludes that the "long-term trend seems still to be one in which the accumulation of capital, combined with the shift towards capital-saving

<sup>18</sup> In Canada currency and active deficits increased by 10 per cent from 1946 to 1948.

<sup>19</sup> No government can be certain of continuous *full* employment, however much it may aim to achieve this goal. But is it not still more difficult to guarantee exact price stability, let alone an exact percentage *increase* in prices? Vickrey has surely not overstated his case when he says that it "may be some time before any such controlled inflation is adopted in any country as a deliberate and explicit policy" (p. 122).

innovations," will tend to drive down the real marginal productivity of capital (p. 122).

Patinkin's paper on the quantity theory is, I feel, only of limited interest. The quantity theory, given certain rigid conditions, is held to be correct. Now Keynes always held that classical theory came into its own under full-employment conditions. He laid down two assumptions under which the quantity theory was valid: (1) full employment, and (2) effective demand will change in the same proportion as the quantity of money. This latter condition would be true under *one* of two assumptions: (1) the propensity to hoard is zero; or (2), if not zero, then the liquidity preference schedule, the investment demand schedule and the consumption function are assumed to have such slopes that the aggregate demand will increase in the same proportion as the increase in the quantity of money.

Patinkin, however, argues that two conditions, and only two, are necessary to make the quantity theory valid: (1) full employment, and (2) absence of "money illusion."<sup>20</sup>

Patinkin thinks that his position presents a significant difference between himself and Keynes, but I question that this can be maintained. He admits that Keynes was quite right in insisting that the validity of the quantity theory was connected with the condition that effective demand must increase in the same proportion as the quantity of money. As Patinkin puts it, this latter condition is indeed a necessary consequence of his own crucial assumption with respect to the absence of any money illusion.

Patinkin's chapter, as with everything he writes, deserves the careful attention of economists, and it is possible that I have not done him full justice. At any rate this chapter interests me far less than his earlier very able contributions. With respect to this chapter, and indeed all the others as well, it is far more difficult to assess in a balanced way a book containing chapters by fifteen authors than a book of fifteen chapters by one author. All in all the book is an interesting and useful contribution to post-Keynesian economics.

<sup>20</sup> True, he takes cognizance (as did also the classicals) of the special condition that there must be no distributional effects such as might account for forced saving.

## COMMUNICATIONS

### Elasticities, Cross-Elasticities, and Market Relationships: Comment

Over recent years, a good deal of confusion and inconsistency has grown up around the concept of cross-elasticity of demand, a fact which must have occasioned distress to all those who have attempted to follow the literature of value theory during this period. We are, therefore, all indebted to R. L. Bishop for his valuable and timely article on the subject.<sup>1</sup> In particular, Bishop has rendered us two important services. Firstly, he has provided an admirable survey of the variety of opinion surrounding the concept of cross-elasticity of demand, and has brought into the open the deep cleavages which exist. Secondly, he has recalled our attention to the fundamental fact that "the concepts in question are elasticities and cross-elasticities of demand."<sup>2</sup> In this connection, he shows how many writers have attempted to qualify the concept of cross-elasticity by reference to supply factors and directs our attention to "the profound anomalies that are implied by their half-breed supply-demand elasticities."<sup>3</sup> Unfortunately, however, when Bishop embarks upon positive prescription, we find that the basic confusion remains embedded in his own work.

Clearly, if we are to use cross-elasticities to define market relationships, it is essential that we have a clear conception of what they do in fact measure. Otherwise, any superstructure of market classification based upon them will be erected on shifting sands. Therefore, in this note, we shall confine ourselves to the fundamental question: What does cross-elasticity of demand measure?

The concept of cross-elasticity of demand with which we shall be concerned is that of its original sense: the ratio of the percentage loss of output by any firm  $j$  to the initiating percentage *reduction* in price by another firm  $i$ , all other prices being assumed unchanged; so that  $E_{ji} = p_i \partial q_j / q_j \partial p_i$ , where  $p_i$  is the price of firm  $i$  and  $q_j$  is the output of firm  $j$ , and where  $\partial p_i$  is considered in the negative sense.<sup>4</sup> This cross-elasticity is, then, the same as Triffin's first (sub-

<sup>1</sup> "Elasticities, Cross-Elasticities, and Market Relationships," *Am. Econ. Rev.*, Dec. 1952, XLII, 779-803.

<sup>2</sup> *Ibid.*, p. 781. Bishop's italics.

<sup>3</sup> *Loc. cit.*

<sup>4</sup> Strictly speaking, it has not been usual in defining cross-elasticity to restrict the direction of the price-change. However, I believe most readers will accept, without quotation of chapter and verse, that invariably discussion has proceeded, either explicitly or tacitly, in terms of a price-cut. For the rest, it seems to have been assumed automatically that the same characteristics would attach to a rise in price. However, a price-cut involves the reduction of one price in relation to all others, whereas a price-rise implies the reduction of all other prices in relation to one. Where substitution in all directions is not continuous (see the latter part of footnote 10), the symmetry between a price-cut and a price-rise will not generally hold. For a more detailed discussion of this point, see my "A Kinked Demand Curve for Monopolistic Competition," *Econ. Record*, May 1953, XXIX, 19-34.

stitution) coefficient, except that it specifically excludes the case of a rise in  $p_i$ .

It should be noticed that this definition of cross-elasticity of demand makes no reference to the supply restrictions which Bishop rightly deplores. Moreover, it is clear that this cross-elasticity, freed from supply qualifications, is a coefficient of substitution. Indeed, this was precisely the purpose of its original introduction by Kaldor.<sup>8</sup> The aim was to provide a more satisfactory criterion of substitutability than that of physical description of the products. It was to meet the difficulty, as Triffin puts it, that "the competition may be keener between Ford and Rogers Peet than between Ford and Rolls-Royce."<sup>9</sup> And certainly this conception of cross-elasticity has persisted in the textbooks, at least in certain chapters of them. Thus, for example, Stigler tells us: "The cross-elasticities provide a convenient index of the readiness with which consumers substitute one commodity for another."<sup>10</sup>

In the classical tradition, it was held (correctly) that two criteria are necessary to define market relationships: (1) a substitution criterion, and (2) a numbers criterion. Triffin (as well as Kaldor, Stigler and others) was clearly correct in using price cross-elasticity as a substitution criterion; and, on this score, Bishop's criticism—that Triffin's interpretation of cross-elasticity is "one-sided" because it takes no account of the "scale of numbers"<sup>11</sup>—completely misses the mark. Indeed, it is from attempts to use price cross-elasticity as a numbers criterion, and in particular to use it as *the* criterion of oligopolistic interdependence, that confusion has inevitably arisen. High cross-elasticities are certainly a *necessary* condition for oligopoly, since price interdependence requires that the products concerned should be good substitutes. But further conditions are needed to satisfy the numbers requirement.

Cross-elasticity of demand as we have defined it, *i.e.*, as it is usually defined, is then, a coefficient of *substitution*; it is not a function of numbers. In fact, the supply restrictions upon cross-elasticities, which Bishop convincingly rejects, were allowed to intrude in the belief, mistaken as we shall see later, that this attached a numbers significance to cross-elasticity of demand. Yet Bishop, having abandoned these quite inappropriate supply qualifications, says: "The truth of the matter, of course, is that the value of  $E_{ji}$  depends on *both* the scale of numbers and the scale of product homogeneity-heterogeneity; and consequently it is not a sure clue to either, separately."<sup>12</sup> While this displays a commendable spirit of compromise, the conclusion is not correct. Indeed, if it were correct, then cross-elasticity of demand, not being a unique measure of anything, would have little purpose to serve and might be rejected out of hand.

The cross-elasticity of demand  $E_{ji}$  is a measure of the rate at which  $j$  cus-

<sup>8</sup> "Mrs. Robinson's 'Economics of Imperfect Competition,'" *Economica* (N. S.), Aug. 1934, I, 335-41; and "Market Imperfection and Excess Capacity," *Economica* (N. S.), Feb. 1935, II, 33-50.

<sup>9</sup> R. Triffin, *Monopolistic Competition and General Equilibrium Theory* (Cambridge, Mass., 1941), p. 88.

<sup>10</sup> G. J. Stigler, *The Theory of Price* (New York, 1947), p. 89.

<sup>11</sup> *Op. cit.*, pp. 785-89.

<sup>12</sup> *Ibid.*, p. 787. Italics in original.

tomers will move to product  $i$  in response to a reduction in the price  $i$ . From the indifference function of each individual  $j$  customer, we may deduce the rate at which each such customer will substitute product  $i$  for  $j$ . By summation, in the manner of the Marshallian demand schedule, we may construct a cross-demand schedule ( $q_i$  plotted against  $p_i$ ) for the product  $j$  with respect to the price of product  $i$ . This will give us the rate at which  $j$  customers as a whole will substitute product  $i$  for product  $j$ .<sup>10</sup>

But, whereas cross-elasticity of demand is a function of the rate at which consumers will substitute the one commodity for another, own-elasticity of demand depends, not only on the rate of substitution between individual products, but also upon the *number* of substitutes available. After all, as Bishop himself stresses, own-elasticity of demand is a measure of "total substitutability," *i.e.*, the substitutability between the "own" product and the collectivity of products which make up its competitive field. It is surely obvious that any change in the variety or number of products within this competitive field will affect the value of own-elasticity of demand.

Let us pursue this question of the relationship between cross-elasticity and own-elasticity of demand further. For this purpose, we need to make a slight change in our definition of elasticity and cross-elasticity. Usually, elasticities are expressed as a ratio between a percentage price change and a percentage *output* change. However, under differentiated competition, units of output, as between firms, are not strictly commensurable. Therefore, instead of output, we now insert *sales*, *i.e.*, output in value terms; and to signify the change we substitute in our elasticities  $Q_i$  for  $q_i$ . We further suppose that the total value of sales lost by any firm  $j$  in response to a unilateral price-cut by  $i$  will accrue as additional sales to  $i$ . (This assumption is equivalent to neglecting the income effect of the price change.) Then, on this basis, own-elasticity of demand may be readily expressed in terms of the array of cross-elasticities:

<sup>10</sup> Cross-elasticity of demand for an individual consumer, which we may denote by  $E_{ji}$ , is given by  $E_{ji} = k_i \sigma - k_i Y_{ji}$ , where  $k_i$  is the fraction of income spent on  $i$ ,  $\sigma$  is the elasticity of substitution between  $i$  and  $j$ , and  $Y_{ji}$  is the income elasticity of demand for product  $j$ . (See J. R. Hicks, and R. G. D. Allen: "A Reconsideration of the Theory of Value," *Economica* (N. S.), May 1934, I, 201-2.) Then, if we neglect the income effect as we are entitled to do when only a fraction of income is spent on any one commodity, individual cross-elasticity of demand is a simple function of elasticity of substitution,  $\sigma$ . Firm cross-elasticity is the weighted average of these individual cross-elasticities.

Actually, this is not the whole story, because in conditions of pure monopolistic competition, if I may coin an expression, the process of substitution will not generally be continuous, *e.g.*, as in the case of substitution of one brand of toothpaste for another. Rather, in this situation, there would be a critical price ratio at which total expenditure on the one product (abstracting from income effects) would be shifted to the other. In terms of indifference curves, this case would be represented by a curve of infinite elasticity of substitution, but with a slope (marginal rate of substitution?) different from unity. Here, resort must be had to indirect methods to translate consumer preferences into cross-elasticities. Incidentally, it will be noticed that these cross-elasticities (finite) are more appropriate indices of substitutability than the elasticities of substitution which are infinite. However, this is a digression. My purpose here is only to show that cross-elasticity is directly related to the theory of consumer choice which, of course, is essentially a study of substitution. After all, the concept of cross-elasticity was born in the London School which, at the same time, was also nurturing the neo-Paretian analysis.

$$\begin{aligned}
 -E_{ii} = & \frac{Q_j}{Q_i} E_{ji} + \frac{Q_k}{Q_i} E_{ki} + \dots + \frac{Q_n}{Q_i} E_{ni} + \frac{Q_o}{Q_i} E'_{io} \\
 & + \frac{Q_p}{Q_i} E'_{pi} + \dots + \frac{Q_s}{Q_i} E'_{si},
 \end{aligned} \quad (1)$$

where the  $E_{ji}$  denote cross-elasticities of the first order of importance and the  $E'_{oi}$  denote those of second-order significance.<sup>11</sup>

For convenience, we assume: (1) that the first-order cross-elasticities are all equal, *i.e.*, the  $E_{ji} = E_{ki}$ , and that the second-order cross-elasticities are all zero; and (2) that all products are substitutes, *i.e.*, none are complementary, so that we may take the arithmetical value of elasticities without ambiguity, and so speak of larger or smaller elasticities without regard to sign. Then, on these assumptions, we may rewrite (1) in the simpler form:

$$E_{ii} = \frac{Q_j + Q_k + \dots + Q_n}{Q_i} E_{ji} \quad (2)$$

$$\text{or, } E_{ii} = \frac{\text{Non-}i \text{ Output}}{\text{Output of } i} E_{ji} \quad (3)$$

Now, if the concept of numbers means anything in relation to a group, it expresses a relationship between firm output and total group output. If numbers are large, the firm's share in group output is small; and conversely if numbers are few. Then it is easy to see from (3) that any change in firm  $i$ 's share of group output will, other things remaining the same, involve a corresponding change in own-elasticity of demand,  $E_{ii}$ . Take, for example, a decrease in numbers, *i.e.*, an increase in  $i$ 's share of the market. This must necessarily involve a decreased elasticity of demand for firm  $i$ , since, *ex hypothesi*, the size of the market from which sales may be attracted has contracted in relation to  $i$ 's own output. Although own-elasticity of demand depends on numbers in this fashion, cross-elasticity does not. This is so because the sales lost by any firm  $j$  in response to a price-cut by another firm  $i$  are related to the output of firm  $j$ . Hence, so long as the proportions between  $j$  customers of various degrees of sensitivity are unchanged, any increase (or decrease) in  $i$ 's share of the market will affect the numerator and denominator of the cross-elasticity ratio in the same proportion.

The reader will not have failed to notice that our equation (3) expresses the same relationship as Bishop's so-called "numbers equivalent" which he writes  $-E_{ii} = (n_i - 1)E_{ji}$ . Also, it will be clear that the essential difference between

<sup>11</sup> Cf. Stigler, *op. cit.*, p. 235: "The group may be defined as all firms whose cross-elasticities are greater than some constant. This constant can best be chosen in any particular problem by ranking the cross-elasticities and considering a group to end where a considerable gap appears in the array of cross-elasticities." Interestingly enough, most economists would accept this procedure. But many do not seem to realize that in accepting this technique, based as it is on the notion of a gap in the chain of substitutes, they are accepting cross-elasticity as a valid substitution criterion.

Bishop's interpretation of this relationship and that given here is that, whereas Bishop treats cross-elasticity of demand as the dependent variable when  $n_i$  changes, for us it is own-elasticity which is dependent. Indeed, Bishop's argument always proceeds on the basis: *Given* own-elasticity of demand, then cross-elasticity will be large or small according as numbers are small or large. But, it is fair to ask, by whom or what is own-elasticity of demand given—apart from by the textbooks—except in relation to the closeness and the number of the individual products available as substitutes, the "total substitutability" as Bishop describes it. It is cross-elasticity which measures the closeness of the substitutes and is therefore the natural substitution criterion. Own-elasticity is then determined by the order of the cross-elasticities and the number of them.

This argument is best demonstrated by considering the case of homogeneous competition. In this case, cross-elasticity of demand will be the same whether there are two firms (pure duopoly) or 2,001 firms (pure competition), since cross-elasticity in both situations will be equal to the reciprocal of the percentage price-cut.<sup>12</sup> But own-elasticity of demand for the pure competitor will be much higher than for the pure duopolist. Although, of course, both elasticities may be made as large as we like, if we make the price-cut small enough, elasticity of demand under pure competition will always be much the higher for any given price-cut, however small. Indeed, if the firms in each group were symmetrical, own-elasticity in the pure-competition case would be just 2,000 times that in the duopoly case.<sup>13</sup> It follows as a corollary that, with any given price-cut, we can always make own-elasticity of demand, but not cross-elasticity, as large as we like by making our firm's share of total output small enough, *i.e.*, by making numbers large enough.

At the other end of the scale of substitutability, our interpretation of cross-elasticity is also the natural one. By contrast, Bishop's conception of the relationship between cross-elasticity and own-elasticity leads to paradoxical results when applied to "pure monopoly." In this case, own-elasticity of demand will be finite, while cross-elasticities will be very small or zero. Hence, their ratio—Bishop's "numbers equivalent"—will be very large indeed. And this Bishop frankly confesses, for he says that in this case the numbers equivalent "must be more or less *uniformly* high with respect to all other firms, without exception."<sup>14</sup> At best, this can hardly be said to be a happy choice of terms. Triffin, on the other hand, would describe this situation as one in which the cross-elasticities of demand (measuring the substitutability of products) are more or less uniformly *small*. Surely this is a more apposite description. This becomes obvious as soon as we put the question: Is pure monopoly non-oligopolistic because of the absence of close substitutes (failure of the substitution condition) or because of large numbers (failure of the numbers condition)?

<sup>12</sup> Since for homogeneous competition  $\delta Q_i/Q_i$  will always be equal to unity.

<sup>13</sup> It will be remembered that we have excluded supply conditions from our definition of cross-elasticity. Hence, a price-cut by a purely competitive firm must be considered, conceptually, to capture the whole market.

<sup>14</sup> *Op. cit.*, p. 800.

It should be stressed that nothing in the foregoing argument depends on Mrs. Robinson's notion of products "coming closer together" or of new firms "coming in between" old firms, a conception which has been the subject of debate from time to time.<sup>15</sup> Mrs. Robinson, it will be recalled, argued that:

. . . if the new firms were set up, so to speak, in between the old firms (either geographically or in respect of special qualities which appeal in various degrees to different customers) . . . the difference, from the point of view of buyers, *between any one firm and the next* would thus be reduced, the customers of each firm would become more indifferent, and the elasticity of demand would be increased.<sup>16</sup>

This is a valid argument.<sup>17</sup> But it is important to recognize that there are two effects involved here: not only an increase in numbers per se (the numbers effect), but also a change in the distance between individual products, as is indeed stated in the italicized phrase (the substitution effect).

The conceptual distinction between the substitution and numbers effects becomes clear when we consider that the notion of products "coming closer together" in no way depends on increasing numbers. It could equally well come about, for example, as a result of an all-around reduction in transport costs or of a technically inspired move towards greater standardization of existing products. In these cases, numbers would remain unchanged; but cross-elasticities, and hence own-elasticity, of demand would increase. On the other hand, an increase in numbers need not *logically* imply any change in the degree of substitutability between individual products, as for example when the entry of new firms does not alter the "distance" between any two products, as indeed will always be the case with homogeneous competition.

However, in the real world of differentiated products, a change in numbers will almost certainly involve some rearrangement of consumers' preferences;<sup>18</sup> and hence some change in cross-elasticities of demand. This effect will usually be in the same direction as, but remains conceptually distinct from, the numbers effect. The important fact is that cross-elasticities change, not in virtue of the change in numbers as such, but because of the change in substitutability which will usually be *associated with* a change in the product-structure of a group.

It is from confusion of these two effects, it seems to me, that has arisen the

<sup>15</sup> In particular, during the 1938 debate between Chamberlin and Kaldor in the *Quart. Jour. Econ.*

<sup>16</sup> Joan Robinson, *Economics of Imperfect Competition* (London, 1933), p. 101. My italics.

<sup>17</sup> I.e., given Mrs. Robinson's condition: "If the new firms were set up in between the old firms." The fact that an increase in the numbers of firms *may* involve merely an increase in area and not in density—a point made much of by Chamberlin—is irrelevant to the *particular* argument in question.

<sup>18</sup> Strictly speaking, an increase (say) in the number of firms will not of itself alter consumers' preferences *as between* the old products. Rather it will establish between the old firms and the new firms, and between the new firms themselves, a new set of cross-elasticities, which may be larger or smaller than existing cross-elasticities. (In Mrs. Robinson's example they will, of course, be larger.) Nevertheless, the change in numbers will imply some redistribution of consumers between firms and this will, in general, affect the pre-existing cross-elasticities also.

"third force" view, as represented by Bishop, for example, that cross-elasticity is a function both of product substitutability and numbers. The clear implication of this view is that the effect of a change in numbers upon cross-elasticity cannot be theoretically determined, but is an empirical question only to be answered by reference to the facts of any given situation. But this approach is theoretically misleading. In so far as a change in the number of firms (products) changes the opportunities for substitution and the pattern of consumer attachments, it changes the pattern of cross-elasticities. But the important point is that it changes cross-elasticities via changes in substitutability or, if you like, via changes in the complex of consumers' preferences which theory has almost universally taken as data. However, there is no essential connection between such changes and changes in numbers in the sense used throughout this paper and by Bishop. This latter conception of numbers relates to the ratio of firm to group output, and changes in this ratio may clearly occur independently of any change in consumers' scales of preference.<sup>19</sup>

Since the matter is of fundamental importance for formal analysis, perhaps it is well to attempt to pinpoint the source of the long-standing confusion of cross-elasticity with numbers. In the old scheme—when commodities were different in kind, not merely in degree—numbers were determined as a simple arithmetic relationship between the capacity of any one firm and the total market supply of the commodity in question. Thus, if a firm's capacity were small compared with total output, the situation would be one of pure competition, because any additional supply it could add to the market would, when "spread over" that market, affect other suppliers only to a negligible degree. On the other hand, if a firm's potential capacity were large in relation to total supply, oligopolistic implications would arise.

This conception was adapted by Chamberlin to the general case of monopolistic competition. Thus, in a formulation which he still finds "unobjectionable,"<sup>20</sup> he defined the "large group" as follows:

Specifically, we assume for the present that any adjustment of price or of "product" by a single producer spreads its influence over so many of his competitors that the impact felt by any one is negligible and does not lead him to any readjustment of his own situation.<sup>21</sup>

It is but a short step from here to those "half-breed supply-demand cross-elasticities" which Bishop denounces, a step, incidentally, that Chamberlin himself

<sup>19</sup> Perhaps the distinction is best made clear by an illustration. Imagine that a sectional increase in  $i$  customers' incomes results in a doubling of purchases from firm  $i$ , the outputs of all other firms in the group and the whole complex of consumer preferences remaining unchanged. Own-elasticity of firm  $i$  will approximately halve. By contrast, the cross-elasticities will not be affected, except in so far as the alteration in income shifts the margin at which substitution takes place. Since, when we speak of substitutability, we mean substitutability at the margin, there is clearly a sense in which we may take cross-elasticity, but cannot take own-elasticity, as data given by the total map of consumers' preferences.

<sup>20</sup> A Comment on Bishop's article, *Am. Econ. Rev.*, Dec. 1953, XLIII, 913.

<sup>21</sup> *Theory of Monopolistic Competition*, 3rd ed. (Cambridge, Mass., 1938), p. 83.

seems to have taken in his more recent work.<sup>22</sup> It would take me too far from the present purpose to discuss here the validity of Chamberlin's formula as a numbers criterion. But I do wish to dispute its relevance to the concept of cross-elasticity of demand.

Let us consider Chamberlin's formula in terms of a price-cut. Now it is clear that we must invoke some sort of supply restrictions if we are to make any sense at all of his conception of the effect of a price-cut being "spread over" a given number of rival firms. This is so because, theoretically, we can make the increment of sales to the price-cutting firm, and therefore the amount to be taken from rivals, as large (small) as we like if we make the price-cut large (small) enough. This follows immediately from the fact that, *ex hypothesi*, the relationship between a price change and a quantity change under conditions of monopolistic competition is always a *finite* one. Therefore, the increment of sales accruing to a price-cutting firm, and hence the amount taken from each rival, cannot be determined, unless appeal is had to some consideration of supply.<sup>23</sup> Now it is possible to argue that the amount by which a "large group" seller may increase sales is limited by supply factors. But, the smaller is a firm's potential capacity, the smaller need be the price-cut to take up this capacity; and since cross-elasticity of demand is the ratio of the quantity change to the price change, its value will not be affected by any considerations of supply.

It is perfectly true, of course, that, with given rates of substitution, the greater the *number* of competitors, "the less the price reduction necessary to secure any desired increase in the sales of A . . . And . . . the less the effect of any given increase in the sales of A on the sales of rival products. . . ."<sup>24</sup> However, since the effect of (say) greater numbers is (1) to reduce the price-cut required to achieve any predetermined result; and (2) to reduce the amount which it is necessary to take from any individual competitor, the cross-elasticities of demand are unaffected, because the numerator (percentage sales loss) and the denominator (percentage price-cut) are changed in the same proportion. Hence, *cross-elasticity of demand is independent of numbers*.

Since the idea of cross-elasticity as a measure of substitutability seems simple and straightforward enough, or at least so it has appeared to a good many writers, it may well be asked why its general adoption has been so strenuously resisted. The answer, I believe, is to be found in the implications which stem from its acceptance.

<sup>22</sup> For example, "Measuring the Degree of Monopoly and Competition," a contribution to *Monopoly and Competition and Their Regulation*, E. H. Chamberlin, ed. (London, 1954), esp. Pt. 4, pp. 262-67. More recently, however, he has retreated from this position and reverts to his original definition of isolation which I have quoted in the text above. He now says, "It should be noted that zero cross-elasticities, though sufficient, are not necessary to isolation in this sense; and also that in the vexed case of pure competition, however one may rule as to the value of the cross-elasticity coefficient, a single seller is isolated in the sense described." (Comment on Bishop's article, *op. cit.*, p. 913.) In short, he now admits there is no necessary relation between his large-small numbers criterion and cross-elasticity of demand.

<sup>23</sup> Bishop is overgenerous in saying that attempts to qualify cross-elasticities with supply restrictions have been confined to homogeneous competition. *Op. cit.*, p. 781.

<sup>24</sup> G. J. Stigler, *op. cit.*, p. 235.

The argument advanced in this paper is essentially the same as that put forward by Kaldor in the 1938 controversy with Chamberlin. During that debate, it will be recalled, Kaldor regarded as "crucial" the proposition that "a shift of the [demand] curve to the left will increase the elasticity of demand at the equilibrium level of output. . . ."<sup>25</sup> And since such a shift to the left is associated with an increase in the number of firms, the "curves become more and more elastic with an increase in numbers. . . ."<sup>26</sup> Chamberlin rejected this proposition and reiterated his opposition to the idea that "differentiation of the product (in my sense) may be exorcised by the process of merely increasing the number of firms."<sup>27</sup>

Chamberlin was perfectly correct in asserting the latter, as he was wrong in rejecting the former, for it is clear that, no matter how small may become the output of a firm as the result of increasing numbers, there may still be a proportion, and even a constant proportion, of customers who will be prepared to continue paying something more for the product of that particular firm than for any of its substitutes. Product differentiation remains; and upwards elasticity of demand, *i.e.*, elasticity in respect of a price-rise, does not necessarily increase as output contracts in relation to group output. Indeed, as output contracts the firm may be left with only a hard core of its most loyal customers. Elasticity to a price-rise may actually decrease.

Thus, we arrive at the position where, as a firm's output contracts in relation to group output, its downward elasticity,  $E^d$ , increases (*vide* Kaldor), while its upward elasticity,  $E^u$ , may be constant or even decreasing (*vide* Chamberlin). And both points of view are correct. The seeming contradiction, which has persisted from the 1938 debate to Bishop's work in 1952, is only a contradiction so long as we cling to the preconception, which has no real basis except in the economist's penchant for simple stable equilibria, that  $E^d$  should approximate to  $E^u$ , *i.e.*, that the demand curve under differentiated competition should necessarily be smooth.

On a subject pregnant with misunderstanding, it is perhaps well to conclude by stating briefly what is *not* implied in my argument here. Firstly, although I have argued that Triffin's use of price cross-elasticity as his substitution criterion is, with the qualification mentioned, unexceptionable, I would not be prepared to accord the same support to his numbers criterion. The so-called "quantity cross-elasticity" by which he distinguishes between large and small numbers is purely definitional. At no point does Triffin postulate the conditions of demand or supply that determine the value taken by his numbers coefficient. Secondly, in so far as the degree of monopoly or competition may be expressed in terms of elasticities of demand, which is certainly not so in the case of oligopoly for instance, I believe it is own-elasticity, measuring total substitutability, which is the appropriate index. I would agree with criticisms of Triffin's suggested use of cross-elasticity in this connection. It should, I think, occasion

<sup>25</sup> N. Kaldor, "Professor Chamberlin on Monopolistic and Imperfect Competition," *Quart. Jour. Econ.*, May 1938, LII, 518.

<sup>26</sup> *Loc. cit.*

<sup>27</sup> Reply to Kaldor, *Quart. Jour. Econ.*, May 1938, LII, 531.

no great surprise that a measure of monopoly or competition which involves both substitutability and numbers is more appropriate than one merely measuring substitutability.

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### Reply

With respect to my own article, the principal issue raised by Hieser's interesting paper concerns the comparative merits of  $E_{ii}$  and  $E_{ji}$  as indices of the homogeneity or heterogeneity of products. In commenting on this question, I shall limit my discussion for the most part to the very special case of isolated, symmetrical groups of  $n$  rival firms.

In such cases, Hieser and I agree that the ratio  $-E_{ii}/E_{ji} = n - 1$  reflects the "numbers" criterion to at least a fair approximation (p. 376). We also agree that the classification of market relationships requires a "substitution" criterion as well. We differ, however, in that I prefer the own elasticity  $E_{ii}$  for this purpose, while Hieser prefers the cross-elasticity  $E_{ji}$ . At bottom, this difference between us is definitional; but it is well worth debating which definition is the more useful analytically. For my part, I acknowledge that the cross-elasticities  $E_{ji}$  and  $E_{ij}$  indicate the substitutability between the  $i$ th and  $j$ th products; and Hieser seems to agree, in turn, that  $E_{ii}$  reflects what I call the "total substitutability" between the  $i$ th product and all others taken together—or all others within the relevant group when there is no significant substitution between the  $i$ th product and others outside that group (p. 375). We would further agree, I take it, that both "total substitutability" and the "individual substitutability" between pairs of products have their own respective significances. It remains, therefore, for me to indicate my reasons for preferring  $E_{ii}$  as the more useful substitution criterion for the specific purpose of classifying market relationships. I am especially grateful for the opportunity to do so; for Hieser's note has made me belatedly aware that I have never given anything like a complete explanation of my choice in this respect.

In reply to the first round of criticisms of my article, it was necessary for me to emphasize that a remarkably close approach to pure competition is consistent with very large magnitudes of  $E_{ji}$ , provided only that  $-E_{ii}$  is all the larger—such that  $-E_{ii}/E_{ji} = n - 1$  is also large.<sup>1</sup> At the same time, I also observed that "cross-elasticities are not necessarily high in atomistic competition."<sup>2</sup> This goes to the heart of the question as to the relative merits of  $E_{ii}$  and  $E_{ji}$  as the preferred criterion of product homogeneity or heterogeneity in the context of market classification. I must now emphasize that a remarkably close approach to pure competition is also consistent with very small cross-elasticities, provided that  $-E_{ii}$ , while itself very large, is small relative to  $n$ —such that  $-E_{ii}/(n - 1) = E_{ji}$  is small.<sup>3</sup>

<sup>1</sup> "Reply," *Am. Econ. Rev.*, Dec. 1953, XLII, 916-17.

<sup>2</sup> *Ibid.*, p. 917, n. 2 (original italics).

<sup>3</sup> There is a quite intriguing symmetry—and complementarity—between this reply and my former one. There I had to defend my "numbers" criterion against the view, originally taken by Chamberlin among others and subsequently maintained in a somewhat different

Suppose, for example, that there are 1,000,001 firms in an isolated, symmetrical group, and that each  $E_{ii} = -5,000$ . For all practical purposes this hypothetical industry is a purely competitive one, because  $n$  is very large and the other-prices-constant demand facing each individual firm has an elasticity hardly distinguishable from infinite. Yet  $E_{ji} = -E_{ii}/(n-1)$  is .005! With still higher numbers and the same  $E_{ii}$ , obviously, cross-elasticities can be made indefinitely small.

Now, if  $E_{ji}$  is specified as *the* measure of "substitutability," in accordance with Hieser's preference, it must at least be conceded that a situation can closely approximate pure competition even though the products of *pairs* of rival firms are only very weakly substitutable. In short, while high *total* substitutability is a necessary condition in order that pure competition may be closely approached, high *individual* substitutability is not. True, we speak of product "homogeneity" as a necessary condition for pure competition; but, in my view, the relevant version of that concept concerns the substitutability among *all* the products in the relevant group, rather than that between merely successive pairs of them. That is why I prefer to specify the "homogeneous-product" requirement of pure competition (and pure oligopoly) in terms of a high magnitude of  $E_{ii}$ , rather than in terms of  $E_{ji}$ .<sup>4</sup>

Similarly, when products are significantly heterogeneous or differentiated, this also seems to me to be most reliably reflected by the moderate or low absolute magnitudes of  $E_{ii}$ . Consider, for example, a symmetrical, isolated group of three oligopolists, such that each  $E_{ii} = -10$  and each  $E_{ji} = 5$ . To me, this is a significantly *differentiated* oligopoly, because  $E_{ii}$  is significantly different from infinity. In particular, I would say that there is a much lower degree of product homogeneity in this industry than in the all but purely competitive one where every  $E_{ii} = -5,000$ . Yet Hieser would have to classify this significantly differentiated oligopoly (with  $E_{ji} = 5$ ) as exhibiting a much *greater* degree of product homogeneity than the above situation of near-pure competition (with  $E_{ji} = .005$ ). In my contrasting view, this species of three-

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form by Fellner, that numbers—and, more generally, the distinction between the presence and absence of oligopoly—are reliably reflected by the magnitude of  $E_{ji}$  alone. I am pleased to have Hieser with me on that phase of the argument; and I am also pleased to have his support for Triffin's view and my own—as against Chamberlin and others—that supply restrictions should not be allowed to intrude on the elasticity concepts used in market classification. At the same time, it is Chamberlin and I who are arrayed against Hieser and Triffin as to the relative merits of  $E_{ii}$  and  $E_{ji}$  as the preferred measure of the degree of heterogeneity of products.

<sup>4</sup>To be sure, if the products of a *given number* of rival firms are visualized to become more and more nearly homogeneous, both  $E_{ii}$  and  $E_{ji}$  go to infinity—precisely because their ratio— $E_{ii}/E_{ji} = n-1$ , remains constant. Hence, when products are strictly homogeneous, it would not matter whether we specify that condition as implying infinite  $E_{ii}$  or infinite  $E_{ji}$ . That is why I refer to instances of only near-homogeneity to weigh the comparative merits of the two coefficients as the preferred measure of this aspect of the classificatory problem.

On the other hand, if we visualize *both*  $-E_{ii}$  and  $n$  as approaching infinity,  $E_{ji}$  may be anything at all between zero and infinite—as I also pointed out in my "Reply," *op. cit.*, p. 920. This again emphasizes that a large  $E_{ji}$  is not a necessary condition as pure competition is merely approached.

firm oligopoly would not exhibit as high a degree of product homogeneity until  $E_{ii} = -5,000$  (with  $E_{ji} = 2,500$ ).<sup>5</sup>

While definitions are never right or wrong, but only more or less useful, I do feel that Hieser's fails to accord with the practical distinction that the market classifier wishes to make between substantially homogeneous and significantly differentiated products. At the same time, I should like to acknowledge the paradoxical aspects of the matter. If only because there is a strong tendency to think of substitution as taking place between just *pairs* of products,  $E_{ji}$  does have a certain commonsense priority as a measure of substitutability. On the other hand, if  $E_{ji}$  is to be used in classifying market relationships, at the very least its *significance* as a measure of product homogeneity or heterogeneity within the relevant group requires the additional consideration of the number of firms with which the  $i$ th firm experiences similar cross-elasticity magnitudes.<sup>6</sup> This difficulty is avoided if the concept of total substitutability is used in the first place. This seems to me to be the better choice, both because market relationships involve much more than just the relationships between pairs of firms, and because—for the purpose of market classification—we are less interested in the substitutability between pairs of products than in some measure of the homogeneity or heterogeneity of products as supplied by the entire group of firms that make up any given firm's competitive field.

When Hieser finds a "basic confusion" in my proposals (p. 373), I do not understand him to be accusing me of any error of either fact or logic. Rather my transgression is that of preferring my own measure of substitutability to his. The same thing is true when Hieser characterizes as "not correct" my statement that "the value of  $E_{ji}$  depends on *both* the scale of numbers and the scale of product homogeneity-heterogeneity" (quoted above, p. 374). This would indeed have been incorrect if I had chosen  $E_{ji}$  as my measure of the homogeneity or heterogeneity of products. Actually, of course, I chose  $E_{ii}$ ; so my conclusion still stands with respect to my own definition—as in the expression,  $E_{ji} = -E_{ii}/(n - 1)$ .

Since Hieser and I agree as to the approximate validity of the equation,  $-E_{ii}/E_{ji} = n - 1$  (or the "numbers equivalent,"  $n_i - 1$ , when groups are either asymmetrical, imperfectly defined, or wholly amorphous), it seems to me pointless to argue whether "large numbers" imply a high value of  $-E_{ii}$

<sup>5</sup> Naturally, the fact that this industry is oligopolistic rather than atomistic also implies that the other-prices-constant demand facing any one of the three firms (with  $E_{ji}$  is in some rigid sense a "dependent variable" (p. 377). While I did make use of the price-output decisions—any more than the other-prices-constant demand with  $E_{ii} = -10$  will be the relevant one in our significantly differentiated oligopoly. The other-prices-constant demand function, on which both Hieser and I base our classificatory elasticities, closely approximates the demand relevant for the individual firm's decision-making only in nonoligopolistic situations.

<sup>6</sup> Indeed, Hieser seems to be at least on the verge of conceding this, in the sentence that includes the first part of the quotation from Stigler (p. 380). On the other hand, when he asserts with the emphasis of italics at the end of the same paragraph that "*cross-elasticity of demand is independent of numbers*," I must again reply that it has no classificatory significance in distinguishing near-homogeneous products from significantly differentiated ones except in conjunction with numbers. To the extent that the italicized assertion is intended to have *empirical* validity, I shall comment on that question in just a moment.

or a low value of  $E_{ji}$ .<sup>7</sup> Similarly, I do not wish to commit myself to any hard and fast theory as to the actual effect on the  $E_{ii}$  and  $E_{ji}$  magnitudes of any change in the number of active firms in a particular industry.<sup>8</sup> It is true that I used phraseology such as this in my article (p. 786): "For any given value of  $E_{ii}$ , the larger is  $n$  . . . the smaller is  $E_{ji}$ ." Again (p. 798), I spoke of the implications for  $E_{ji}$  "as differentiated products with given substitutability [*i.e.*, with given magnitudes of  $E_{ii}$ ] are produced by larger and larger numbers of firms." But I also referred to this assumed constancy of  $E_{ii}$  as an "artificial assumption" (p. 798, n.); and in the remainder of that footnote I briefly indicated reasons why  $E_{ii}$  might actually be expected to increase and  $E_{ji}$  to decrease when the number of firms increases in some particular industry.<sup>9</sup>

More generally, such effects will obviously depend on the particular causes of the increase in the number of firms and the particular details of the consumers' tastes for the changing number and variety of available products. Hieser does not explicitly reject this view—indeed, he makes some concessions to it; but he also characterizes it as "theoretically misleading" (p. 379), on the ground that "a change in the number of firms . . . changes cross-elasticities via changes in substitutability." In Hieser's own terminology, however, this proposition is a definitional tautology; for he defines and measures substitutability only in terms of cross-elasticities. In the same context, Hieser also seeks to equate given consumers' preferences with given cross-elasticities but not with own elasticities. In my own view, all of the  $E_{ii}$  and  $E_{ji}$  coefficients are exactly on a par as given aspects of any given situation.

One possibility, I concede, is that the  $E_{ji}$  magnitudes might remain the same as numbers change. But even if this were always so as an empirical matter, it would not affect in any way—as I see it—the relative merits of  $E_{ii}$  and  $E_{ji}$  as the more useful classificatory measure of product homogeneity or heterogeneity. In other words, mere market classification, with which I was exclusively concerned, requires no special theory of the comparative statics of

<sup>7</sup> The same issue is also involved when Hieser expresses misgivings as to the implication that, in "pure monopoly," the numbers equivalent must be high with respect to all other firms (p. 377). Apart from the one-member group that the pure monopolist himself comprises, he belongs to no significant group other than that comprising all the firms in the whole economy. But we must still assure ourselves that the presumed "purity" of the monopoly is not compromised by any threat of oligopolistic interdependence with any other firm. It is then a matter of complete indifference, it seems to me, whether we state that condition in terms of uniformly high values of  $-E_{ii}/E_{ji}$  or in terms of uniformly low values of  $-E_{ji}/E_{ii}$ . The former version suggests, at least by analogy, that the pure monopolist is a member of a large-number group embracing all firms in the economy, while the latter implies that he bulks small in the economy as a whole. In this case, furthermore,  $E_{ji}$  must itself be very small, since  $-E_{ii}$  would always be no more than moderately large (significantly different from infinite) as an empirical matter.

<sup>8</sup> Thus I also wish to disavow explicitly the notion, which Hieser attributes to me, that  $E_{ji}$  is in some rigid sense a "dependent variable" (p. 377). While I did make use of the equation  $E_{ji} = -E_{ii}/(n_i - 1)$  to show how cross-elasticity was related to both "total substitutability" and "numbers," I regard this equation as perfectly interchangeable with such transformations as  $-E_{ii}/E_{ji} = n_i - 1$  (which I also used) and  $-E_{ii} = (n_i - 1)E_{ji}$  (which Hieser prefers).

<sup>9</sup> In that footnote, unfortunately, there is a misprint: "ith" in the next-to-last line should be "jth."

actual changes in the numbers of rival differentiated products. It seems to me that Hieser confuses these more ambitious empirical questions with the quite separate problem of just classifying the alternative market relationships that may exist in any given situation. After all, the classificatory problem involves nothing more than identifying certain strategic coefficients that will be consistently related to those aspects of market relationships in which we are analytically interested.<sup>10</sup>

Hieser also believes that, in groups of firms supplying differentiated products, the other-prices-constant demand facing each firm will typically exhibit a highly distinctive kink—such that the downward elasticities (both own and cross) will markedly exceed the upward elasticities. Feeling that Hieser is quite fundamentally mistaken about this, I wish I might discuss the question at the length that it would require. Moreover, this is an issue of greater substantive and empirical significance than the one that I have discussed. But it is also a quite separate question; and limitations of space forbid my pursuing it at this time. Suffice it to say that my foregoing discussion may be interpreted as applying, from Hieser's point of view, to the downward elasticities only, or—when demands are smoothly continuous—to both upward and downward elasticities.

ROBERT L. BISHOP\*

<sup>10</sup> For this reason, incidentally, I am somewhat puzzled that Hieser should criticize adversely one (but only one!) of Triffin's coefficients as being "purely definitional" (p. 381). The only relevant criticism of any such coefficient, it seems to me, must be based on whether it consistently reflects what it is supposed to reflect.

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### The Growth of American Unions: Comment

In a recent article in this *Review*<sup>1</sup> Irving Bernstein advanced persuasive argument, supported by considerable historical evidence, in support of the contention that the future of the American labor movement is quite bright. He cited figures to show that the trend of membership of organized labor was steadily upward during the period 1897 to 1953, not only in absolute terms, but also relative to the increase in the size of the total civilian labor force. This secular trend in "real membership" (membership as a percentage of the civilian labor force) is explained by the operation of four factors: the expansion of the labor force, the growing social acceptability of trade unionism, increasing homogeneity in the working force, and the extension of collective bargaining provisions for union security. His investigations led him to believe that these same four factors are likely to be operating in the future, assuring that organized labor will "grow steadily in the long run." It is apparent that to "grow steadily in the long run" encompasses a very wide range of possibilities, but it is clear from the entire context of the article that Bernstein is optimistic concerning the prospects for the future growth of organized labor. Bernstein also analysed the relation of the business cycle to union growth, and concluded that unionism will "suffer little or no loss in bad times."

<sup>1</sup> I. Bernstein, "The Growth of American Unions," *Am. Econ. Rev.*, June 1954, XLIV, 301-18.

The writer does not contend that Bernstein's inferences with respect to the future growth of organized labor are necessarily incorrect, but in the absence of more adequate analysis than that presented in this study he finds it difficult to accept them with the same degree of confidence as Bernstein. It is quite possible that certain weaknesses in Bernstein's analysis may have misled him into an overoptimistic appraisal.

In his analysis of the growth of the American labor movement Bernstein was concerned entirely with the determinants of the long-run trend and failed to consider the special circumstances which may have conditioned the growth during particular periods. This was especially unfortunate with respect to the period 1933 to 1946, since the highly favorable environment making possible the phenomenal growth of organized labor during these years is unlikely to exist again in the future. By 1933 the stage was set for a great union advance. Population growth and industrialization had created a pool of millions of industrial workers, largely concentrated in the urban centers, and for reasons that are well known these workers were almost entirely unorganized in 1933. Thus a tremendous potential existed at the beginning of this period. Within the climate of opinion toward group action created by the psychological impact of the depression, industrial unionism and the protection of a benevolent government provided the immediate stimuli for union growth. As a result, millions of workers flocked into the unions. The industrial expansion during the second world war created additional millions of jobs in those industries which lend themselves readily to organization, or which were already highly organized, thus providing further impetus to the union movement. In the postwar period, however, the labor movement has lost its momentum. Those segments of the labor force most susceptible to organization had by 1946 been fairly well exploited, and the postwar campaigns to penetrate those areas and those labor groups less amenable to unionization have met with only very limited success.

The great expansion of employment in the construction industry and in other industries which lend themselves readily to organization has made possible continued growth in the postwar period; however, since the membership increases have been concentrated primarily in these industries, the over-all increase has not been impressive. According to the estimates of the Bureau of Labor Statistics, membership in labor organizations rose from 15 million to between 16½ and 17 million, an increase of only two million or less, during the period 1946-53.<sup>2</sup> During this same period employment in nonagricultural industries increased by 8.5 millions. This experience is in sharp contrast to the war years (1940 to 1945) when organized labor drew 5.9 million additional members into its ranks while employment in nonagricultural industries rose by only 6.2 millions.<sup>3</sup> If the estimates of the Bureau are accepted as a reasonable approximation, the conclusion seems inescapable that a future large-scale union advance will be unlikely unless organized labor is successful in pene-

<sup>2</sup> Membership figure for 1946 from Bureau of Labor Statistics, *Brief History of the American Labor Movement* (Washington, 1947), p. 17. Membership figure for 1953 from BLS, *Directory of Labor Unions in the United States, 1953* (Washington, 1953), p. 2.

<sup>3</sup> Data for employment in nonagricultural industries from *Fed. Res. Bull.*, Apr. 1954, XL, 396. Figure for union membership increase from *Brief History of the American Labor Movement*, *op. cit.*, pp. 17-19.

trating the main citadels of the unorganized groups—i.e., the white collar employees, the trade and service employees, the employees in the smaller firms, the southern workers outside of Richmond and Birmingham, etc. The fact that organized labor has evidenced little ability to expand in these directions in the postwar period indicates that union growth in the future is likely to be slow.

In his discussion of the factors shaping the size of the American labor movement Bernstein failed to consider the long-run changes in the structure of the economy and the consequent changes in the locational and occupational structure of the labor force. As indicated above, these structural changes were one of the most important factors making possible the great union expansion from 1933 to the end of the war. There is, however, no reason to suppose that future structural changes will be so fortuitous from the standpoint of organized labor. The increasing complexity of our industrial system, the expansion of governmental functions, and rising real incomes are bringing about a decline in the manual segment of the labor force, that segment which has furnished the only fertile field for the union organizer. The South, and other areas of anti-unionism, have been expanding industrially relative to those areas which are more congenial to organized labor. The ultimate result of this development may be to lessen greatly the currently strong and widespread anti-union sentiment in these areas, but there is no assurance that this will be the case. A trend toward wide decentralization of industry in the not too distant future is a definite possibility. By shifting industry toward the smaller communities, such a development would increase substantially the organizational problems of the unions. These few examples of possible future developments suffice to indicate the fallacy of assuming that future structural changes will benefit, or will not be detrimental, to organized labor. It is possible, and in the writer's opinion probable, that they will create a continuously less favorable environment for the unions. In any case, conclusions concerning the future growth of the American labor movement based upon an analysis which does not include consideration of this important factor do not warrant a high degree of confidence.

In drawing upon historical evidence to support his contention that unionism "will suffer little or no loss in bad times" the author fails to consider the fact that, in comparison with past decades, a very much higher percentage of union members are now concentrated in those very industries which are most subject to cyclical decline. In 1950, the United Automobile Workers, Steelworkers, and Machinists alone comprised nearly 20 per cent of the total of organized labor, and these same three unions were responsible for over one-third of the total increases in membership which occurred from 1948 to 1953.<sup>4</sup> Also at present a much higher percentage of total membership is made up of workers in the unskilled and semiskilled groups than was true during most of the period under study by Bernstein. These workers are not as likely as skilled workers to retain their membership during periods of layoff. It seems reasonably certain, therefore, that a major decline in the level of business activity would substantially reduce union membership. Even the relatively mild and short-lived recession of 1949 reduced union membership by 200,000.

<sup>4</sup>Source: Bureau of Labor Statistics, *Directory of Labor Unions in the United States, 1949*; BLS, *Directory of Labor Unions in the United States, 1953*.

Future events may confirm Bernstein's optimistic predictions concerning the future growth of organized labor, but on the basis of available evidence it appears more likely that organized labor's advance for some time in the future will be slow. The rate of increase in total membership over the long run is unlikely to equal the rate for the postwar period, with actual setbacks occurring during periods of declining business activity. The future is, however, very uncertain and any conclusions must necessarily be highly qualified and tentative.

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### The Growth of American Unions: Comment

The most challenging conclusion in Irving Bernstein's recent article<sup>1</sup> is that "The conventional monocausal explanation for fluctuations in union membership, the business cycle, is without general validity." Bernstein attempts to show statistically that neither cyclical movements as a whole nor leading components of the cycle (specifically consumer prices, employment, wholesale prices, and industrial production) correlate significantly with unionism. He further concludes that cyclical changes are "useless" in understanding secular expansion in union membership and only sporadically helpful in respect to short-term changes.

On the affirmative side, Bernstein summarizes his findings as follows:<sup>2</sup>

A multicausal system (including the cycle) is necessary to account for the rise of trade unionism. The primary forces that have shaped secular growth are the expansion of the labor force, growing social acceptability of unionism, increasing homogeneity in the working class, and extension of collective bargaining provisions for union security. In the short run membership has expanded sharply as a consequence of wars and very severe depressions. Unions, in other words, have been the beneficiaries of disaster.

Although we do not quarrel fundamentally with Bernstein's pluralistic explanation of union growth, our analysis leads us to conclude that he has minimized unduly the importance of the cycle in a multicausal system. We have assumed, as does Bernstein, that the business cycle may be adequately represented by the four series: cost of living (consumer prices), employment, wholesale prices, and industrial production. Bernstein found no meaningful relationship between "real" union membership (*i.e.*, the number of union members corrected by the size of the labor force) and any of the four economic factors cited.

In our investigation, we deemed it more reasonable to examine the relationship between the growth of unions during a given time period with the characteristics of the business cycle *during the previous time period* rather than, as in Bernstein's analysis, for the same time period. A time lag is reasonable because on the downturn union members rarely fall away at once and on the upturn it takes time to launch organization drives to attract new members and regain former members.

<sup>1</sup> *Am. Econ. Rev.*, June 1954.

<sup>2</sup> *Ibid.*, p. 317.

When a time lag of one year is used, the results are statistically significant. This is shown in the analysis given below.

Let  $Y_t$  = change in union membership during time period  $t$ ,  
 $X_{1t}$  = change in cost-of-living index during time period  $t$ ,  
 $X_{2t}$  = change in employment during time period  $t$ ,  
 $X_{3t}$  = change in wholesale price index during time period  $t$ ,  
 $X_{4t}$  = change in industrial production index during time period  $t$ .

Since the changes in the business cycle during a given time period must be characterized by all four values of  $X_{1t}$ ,  $X_{2t}$ ,  $X_{3t}$  and  $X_{4t}$ , the relationship may be investigated by using a multiple regression model. Let

$$Y_{t+1} = a_0 + a_1 X_{1t} + a_2 X_{2t} + a_3 X_{3t} + a_4 X_{4t}.$$

Using the data presented in the Bernstein article, the following estimated relationship may be obtained.

$$Y_{t+1} = 1.577 + .805 X_{1t} + .207 X_{2t} - .188 X_{3t} + .253 X_{4t}$$

(.45)            (.80)            (.33)            (.19)

The numbers below the regression coefficients are the standard errors of the regression coefficients. On the basis of these standard errors, it appears that the relationship between changes in union growth is not very strongly related to *any one* of the changes in the individual characteristics of the business cycle. However, changes in the cost-of-living index and changes in industrial production individually seem to bear the strongest relationship to changes in union growth.

In order to evaluate the *joint* relationship between the changes in the four characteristics of the business cycle and changes in union growth, the multiple correlation coefficient has been computed. We find that  $R = .481$  and this is significant at the 5 per cent level. This indicates that there is a relationship between changes in the growth of unions during a given time period and changes in the four variables related to the business cycle during the *previous* time period when all of these variables are considered at the same time.

The foregoing analysis does not purport to show that the growth of unions may be completely explained by business cycles. However, it indicates that business cycles are an important factor in explaining union growth and casts serious doubt on the conclusion that cyclical movements and their leading components are "useless" for an understanding of union growth.

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EDGAR M. JACOBS\*

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### Reply

Both these comments express views that appear to differ from mine primarily in emphasis rather than in content. Hence the remarks here run the

risk of exaggerating the area of disagreement, because I shall deal only with substantive differences, starting with Fristoe.

In part, Fristoe may be classed as a "saturationist." This view, in essence, holds that the American labor movement stopped growing at the end of the second world war. The readily organizable areas had been cracked and only tough nuts remained—the white collar worker, the small town, the South, etc. My fundamental objection to this argument has already appeared in the article that led off this controversy and there is no need to repeat. Hence I shall comment now only upon matters not dealt with there.

The first point concerns Fristoe's citation of Bureau of Labor Statistics' estimates of union membership in 1946 (15 million) as compared with 1953 (16½ to 17 million). These figures, it should be noted, are not collected by the Bureau and "are not to be construed as data verified by Government."<sup>1</sup> The 1946 figure (14,974,000) is grossly in error because it estimates CIO membership at 6 million. As is well known, CIO has never revealed its actual membership and the 1946 figure is inflated by about 2 million. By deducting this amount, the result closely approximates Wolman's estimate of 12,980,000. The person formerly responsible for the BLS figure has herself reported 1946 membership (including Canadian) at 13,800,000.<sup>2</sup> The actual rise in the period 1946-1953, therefore, was in the neighborhood of 4 million rather than 2 million.<sup>3</sup>

The second comment deals with what Fristoe calls "the main citadels of the unorganized groups"—white collar, trade and service, small firms, and the South "outside of Richmond and Birmingham." The military image is unfortunate since workers in these areas, though more difficult than others to organize, hardly constitute a fortress against unionism. For the white collar and trade-and-service groups the excellent California statistics are suggestive, though, unfortunately, the series covers only the four years, 1950-1953. The 1,577,900 union members in the state in 1953 were divided between 522,000 in manufacturing (33.1 per cent) and 1,055,900 in nonmanufacturing (66.9 per cent). The industry groups in which white collar and trade-and-service employment predominates had the following number of members in 1953: public utilities (60,700); trade, wholesale and retail (185,400); eating and drinking places, hotels and other lodging places (93,600); motion picture production and distribution, theatres and other entertainment (76,400); miscellaneous services (86,400); and government (49,400). Together they totaled 551,900, or 35 per cent of all members. Further, there is evidence that membership in these groups has been increasing; between 1950 and 1953 it advanced 66,200 or 13.6 per cent.<sup>4</sup>

The notion that small firms are more difficult to organize than large is suspect on its face. Sophisticated students of the labor movement a generation

<sup>1</sup> *Statistical Abstract of the United States, 1954*, p. 235.

<sup>2</sup> F. Peterson, *American Labor Unions* (New York, 1952), p. 62. She reports unbroken growth between 1946 (13,800,000) and her terminal year, 1951 (15,300,000).

<sup>3</sup> The 1953 BLS estimate for the labor movement as a whole is not based upon an inflated CIO figure.

<sup>4</sup> State of California, Department of Industrial Relations, *Union Labor in California, 1950*, p. 13 and *Union Labor in California, 1953*, p. 11.

ago argued precisely the opposite in explaining the failure of the AFL in the mass-production industries. The extraordinarily high level of organization in the San Francisco Bay area, where there are no large firms excepting branch plants of nationwide corporations, is an obvious illustration to the contrary. Even more to the point, however, are the statistics of the National Labor Relations Board in collective bargaining elections. In the fiscal year 1953, for example, the Board conducted 6,050 of these representation elections. Of the 1,368 elections involving units with fewer than 10 employees, unions won 76.7 per cent; of the 3,884 involving fewer than 50, unions won 72.9 per cent; of the 4,720 involving fewer than 100, unions won 71.8 per cent; and of the 5,323 involving fewer than 200, unions won 71.4 per cent.<sup>5</sup> Obviously, unions are finding it worth while to organize small firms.

There is an almost universal tendency to underestimate the extent of union organization in the South, in which Fristoe appears to share. The rapid industrial integration of that region with the nation in recent years has been accompanied by a sharp increase in unionization. In the absence of regional statistics, this conclusion must be supported by a variety of sources. The BLS Community Wage Surveys for 1953-1954, for example, revealed the following percentages of plant workers in manufacturing covered by union contract: Dallas, 61; New Orleans, 63; Memphis, 78; and Atlanta, 57.<sup>6</sup> Nationwide corporations with branch plants in the South have almost invariably brought unions with them in such industries as automobiles, steel, rubber, meat packing, and aircraft. More than 90 per cent of some 40 southern pulp and paper mills are unionized, a higher incidence than any other region except the Pacific Coast. Meyers has estimated the extent of unionization in Texas for April 1953 as follows: manufacturing, 45-50 per cent; railroads, 100 per cent; maritime, 100 per cent; telephone and telegraph, virtually 100 per cent; bus transportation, 66 per cent; and interstate trucking, over 80 per cent.<sup>7</sup>

I concur with Fristoe's comment that the labor movement is relatively more vulnerable to depression now than it was in the past because it has organized the more cyclically sensitive industries and has a higher proportion of unskilled and semiskilled workers. It seems to me, however, that the long-term growth of unions could be affected by these considerations only if the business decline were of unusual severity and duration. There is no evidence that the comparatively mild fluctuations of 1948-1949 or 1953-1954 had any such effect. A laid-off unionist customarily stops paying dues but retains his membership; with re-employment he simply resumes payments. In other words, the relationship between a job and membership is loose in this context.

Finally, the differences between Fristoe's views and my own may be more of tone than of substance. On the basis of historic forces presently at work I would expect *gradual* growth in the size of the U.S. labor movement, leaving

<sup>5</sup> Computed from *Eighteenth Annual Report of the National Labor Relations Board, 1953* (Washington, 1954), p. 109.

<sup>6</sup> Bureau of Labor Statistics, *Wages and Related Benefits, Major Labor Markets, 1953-1954*, Bull. No. 1157, Pt. I, pp. 20, 71; Pt. II, p. 33; Pt. III, p. 20.

<sup>7</sup> F. Meyers, "Factors in the Growth of Unionism in an Industrializing Area," unpublished manuscript, pp. 2-3.

aside such eventualities as war or a great depression. Fristoe appears to consider this a real possibility, but is inclined to emphasize the dark spots. I suspect that he is unduly pessimistic.

Davey, Jacobs, and Monroe are to be commended for suggesting the idea of a one-year lag, which, frankly, did not occur to me. As indicated in the original article, however, there is doubt in my mind as to the appropriateness of correlation analysis in this context; the coefficients were published initially only because they agreed with the results reached with several other methods. Hence the reader was advised to give these coefficients "the weight he thinks they deserve."<sup>8</sup>

This caution applies as well to the correlation analysis of Davey and his associates. The secular trend has not been removed from any of the series employed and all five experienced marked growth. Between 1900 and 1948, industrial production, employment, wholesale and retail prices, and union membership expanded in secular fashion. It is certainly not unreasonable to expect that part, and perhaps most, of the explanation for the correlation is to be found in secular rather than cyclical factors. There is, apparently, a tendency for the coefficient of multiple correlation between time series to rise when the number of series increases. In this connection it is worth noting that none of the individual coefficients was significant, and significance at the 5 per cent level was reached only with a multiple correlation. Finally, the fact that several series move together does not necessarily mean that there is a causal connection between them.

The central argument of substance is this: The trade union is a complex institution only partly sensitive to economic forces. These forces, in turn, are only in part cyclical. Hence it makes little sense to construct a theory of union growth about the cycle. It is far more meaningful to assume a multicausal system (including the cycle) at the outset.

I suspect that Davey, Jacobs, and Monroe would find little to take issue with in what has just been said. Again, the differences are mainly of emphasis.

IRVING BERNSTEIN\*

\*"The Growth of American Unions," *Am. Econ. Rev.*, June 1954, XLIV, 311, n. 10. At this point I must express gratitude to T. E. Southard of the Institute for Numerical Analysis and to Jesse Proctor of the Institute of Industrial Relations, both of U.C.L.A., for checking the Iowa group's computations.

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### Erratum

In the article "The Impact of Labor Disputes upon Coal Consumption," by C. L. Christenson, in the March 1955 number of this *Review*, the second line on page 96 was incorrectly inserted. The first sentence on the page should read, "While railroads with a figure of 132 per cent had duplicated that level in a few single months earlier, they had never before closed a year when reserves had been maintained continuously well above 100 per cent for all twelve months." In the second line of the middle paragraph on the same page, the reference in parentheses should be page 88.

## BOOK REVIEWS

### Economic Theory; General Economics

*The Tools of Social Science.* By JOHN MADGE. (New York: Longmans, Green and Co. 1953. Pp. x, 308. \$4.75.)

*Theory and Method in the Social Sciences.* By ARNOLD M. ROSE. (Minneapolis: University of Minnesota Press. 1954. Pp. xii, 351. \$5.00.)

*On Theory and Verification in Sociology.* By HANS L. ZETTERBERG. (New York: The Tressler Press. 1954. Pp. 78. \$2.50.)

"If we are honest we have to admit that the first century of social science has left us somewhere short of victory." I do not want to be less honest than Mr. Madge is (p. 290) but I should like to submit that some twenty-four hundred years of known social science have left us where we are, and that we shall always be short of victory because there is no end to the battle for knowledge, social or natural. To Madge social science is only a hundred years young because for him it all began with the establishment of the positivist science of sociology.

The "tools of social science" to which Madge devotes his text are "documents" (37 pages), "observation" (27 pages), "the interview" (110 pages) and "experiment" (36 pages). The last is "the apex of scientific method." The "mental experiment is an invaluable dress-rehearsal but it is no substitute for the real thing." There "can often be an orderly progress through search of documents, through observations, through the various forms of questioning, before we are ready for the rigour of experiment" (p. 292). Has he no place for theory at all? Madge is willing to forgive the "theoretically inclined social scientist" because "the desire to explain and to unify social facts, the search for a consistent framework, these cannot be condemned even by the extreme empiricist, because this desire has motivated much valuable empirical research" (p. 291). But "too many hypotheses are based on ideal and logically tidy considerations . . ." (p. 118). "The only safe source of knowledge about human beings is what we can see and hear, and everything else is guesswork." The techniques described by Madge "have therefore an ostensibly behaviourist foundation" (p. 34).

With all his scientific preconceptions, his ultra-empiricism, positivism, and behaviorism, Madge is also an adherent of epistemological relativism, of the coexistence of conflicting truths in social science. To him "the only tenable view is Mannheim's perspectivist view of many truths, each shared only by those who have shared experiences and have agreed between themselves on social aims" (p. 5). And "to postulate an objective social science is to ask for something which is probably unattainable, and may even be undesirable" (p. 6).

A clue to one of the sources of Madge's superstitions can be found in the

singular nouns in the title of his first chapter: "The Method of Social Science." If, instead of proclaiming the coexistence of competing truths, he came to recognize the coexistence of different methods and of several social sciences, he would be less myopic also in other matters and less liable to misguide his readers.

For every misunderstanding and misjudgment in Madge's book a pointed criticism and correction can be found in the book by Arnold M. Rose. The very first chapter (a paper which had won the 1952 prize of the American Association for the Advancement of Science for essays in social theory) contains a good statement on the nature of theory and on the testing of deduced hypotheses. But the best antidote to Madge's scientistic methodology can be found in Chapter 14 on "problem orientation versus method orientation." Rose quotes approvingly the psychologist Maslow, who protested against the "over-valuation of quantification for its own sake," against the "creation of a false and pernicious hierarchical system among the sciences," and against the "creation of a scientific orthodoxy, which in turn (a) tends to block the development of new methods, (b) tends to exclude many problems from the jurisdiction of science and (c) tends to make scientists 'safe' rather than daring" (p. 254).

Rose objects to the "primacy of method," according to which "a body of disciplines known as the 'behavioral sciences'—psychology, sociology, and social anthropology, plus a very small section of political science and economics—is distinct from a body of disciplines known as 'documentary sciences'—history, ethnology, political science, law, institutional and labor economics (the rest of economics is put in a class by itself)" (p. 249). In Chapter 15, on "Generalizations in the Social Sciences," there are, besides excellent comments on determinism versus probabilism, some admirable statements on social science as "a pile of discrete bits of knowledge" as against "an integrated body of propositions." "An adequate system of general propositions is based on an internally consistent set of assumptions and definitions, such that each proposition is capable of being logically deduced from the assumptions and definitions (in the manner of a theorem). The building up of an adequate theory—the shorthand term for the framework of assumptions and definitions as well as the propositions deducible from them—requires a constant interplay between theory and data in which the theory is regularly modified and new deductions are derived from it and once more tested by crucial facts" (p. 263).

Two chapters are explicitly addressed to economists: one on "The Potential Contribution of Sociological Theory and Research to Economics," and another on "A Deductive Ideal-Type Method." In the latter, Rose discusses the convertibility between inductive and deductive propositions and presents a good formulation of the distinction: "Inductive studies test the hypothesis that *b* follows *a* under specified conditions. Deductive studies seek to specify the conditions (assumptions) under which *b* must logically and inevitably follow *a*, and they pose for empirical research the question of the relevance of these conditions to any observable phenomena" (p. 340).

I do not know whether the Roses or the Madges are more representative of present-day sociology. But in this war of the Roses against the Magic of

Scientism the third book under review sides with the former. Hans L. Zetterberg's methodological program proclaims "(1) a concern with theories (definitions and hypotheses) rather than frames of reference (only definitions) . . . (2) verification studies rather than descriptive ones, and (3) a concern with miniature theories rather than an inclusive theory" (p. 13). These "miniature theories," however, should be integrated, not isolated. For, "we explain something by demonstrating that it follows the laws of other phenomena. To ask for an explanation in science is to ask for a theory" (p. 9). Merely "to know the labels of phenomena and to know their distribution is not to explain them" (p. 14).

Zetterberg's observations on operationalism are illuminating. For purposes of verification "only those operational definitions that have a counterpart in a nominal definition are worth while. When verifying a theory, certain measurement devices may very well be worthless [and] irrelevant" (p. 34). The evaluation of the validity of certain operational definitions for a hypothesis formulated in terms of nominal definitions is shown to be one of the most significant steps in the verification of the hypothesis. Zetterberg's book is worth while and relevant for economists.

Fritz Machlup

*The Johns Hopkins University*

*The Share of Wages in National Income.* By ASHOK MITRA. (The Hague: Centraal Planbureau. 1954. Pp. 113.)

Dr. Mitra's dissertation is a valiant attempt to cope with a problem whose fascination is exceeded only by its difficulty. Despite his ingenuity, his efforts are not altogether successful; however, he is to be congratulated upon the real contribution he has made.

The first chapter is a brief survey of classical, neoclassical, and Marxian distribution theories; the second is a critique of the Cobb-Douglas function as an explanation of labor's share. The criticisms of the latter are generally correct, although a better case could be made for the distributive implications of the "function" than the author's arguments would suggest.

Chapter 3 is a quite sound criticism of Kalecki's theory of income distribution. Mitra perceives clearly that Kalecki's theory is perilously close to a tautology; however, he does not point out that the scant empirical content this theory does possess is almost certainly false. Specifically; if it is assumed that the marginal cost curve in the Kalecki model is parallel to the quantity axis (*i.e.*, is "flat"), all other propositions in the theory can be deduced. And as I have argued elsewhere<sup>1</sup> (contrary to Mitra and others) it is not likely that the relevant marginal cost curve—for Kalecki's theory—is flat.

The author is (properly) critical of Kalecki's failure to measure the "degree of monopoly power" directly. Accordingly, in Chapter 4 he attempts to provide such a measure. As Mitra accepts both Kalecki's assumption that the marginal cost curve is flat and Lerner's definition of monopoly power (*i.e.*, price minus marginal cost price), his measure of "degree of monopoly

<sup>1</sup> "Rehabilitation of Partial Equilibrium Theory," *American Economic Review*, May 1952, pp. 191-2.

power" varies in the same direction as the ratio of price to average expenditure on wages plus raw materials. To my way of thinking this begs the question between Kalecki and the "marginalists." Furthermore, the statistical methods used are more courageous than delicate. However, despite these caveats, the chapter's finding that (contrary to Kalecki) the degree of monopoly power has not had a cyclical pattern in Britain, is both important and plausible.

In Chapter 5, the author offers an alternative model (for determining labor's share) whose inspiration he credits to Cournot. This model makes the wage share depend upon the following: the ratio of money wage rates to product prices; the rate of depreciation; the quantities of labor and imports (raw material) per unit of output; the number of competing sellers and the "zero demand price" (the intercept of some unspecified aggregative demand curve with the price axis). He then, in Chapter 6, deduces the effect upon the wage share of varying each of the above parameters; the most interesting of the results is that the wage share will *ceteris paribus* vary in the same direction as the share of raw material producers (*i.e.*, import suppliers).

The derivation of this result (pp. 65-66) is suspect because it involves partial differentiation of the wage-share function without consideration of the constraining identity among gross sales, labor income, nonlabor income, imports (raw materials) and depreciation. Nevertheless I believe Mitra to be close to the right answer—even though I would dispute his method of getting there. This chapter concludes with some applications of the model to the interpretation of British data; these are interesting, but not quite convincing.

It is unfortunate that Mitra's ideas must be strained through a very poor English translation. It is to be hoped that stylistic difficulties do not repel students, as the book merits attention.

MELVIN W. REDER

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*Christian Values and Economic Life.* By JOHN C. BENNETT, HOWARD R. BOWEN, WILLIAM A. BROWN, JR., and G. BROMLEY OXNAM. (New York: Harper & Bros. 1954. Pp. xv, 272. \$3.50.)

This summary volume in the series on Ethics and Economic Life produced by a study committee of the Federal Council of Churches is a miscellaneous collection made up of four parts, one of which discusses the topic announced in the title of the book. Another part comprises six chapters on international economics by William Adams Brown, Jr., who performs the excellent service of showing the pitfalls awaiting those who would rush thoughtlessly into this field with what might appear to be clear and evident Christian solutions. In addition there is a summary of the findings of the five other books in the series and three introductory chapters comprising an historical sketch by Bishop G. Bromley Oxnam.

The work most pertinent to the topic of Christian Values and Economic Life is that contributed by John C. Bennett, professor of Christian theology and ethics. Bennett advocates a middle way between the extremes of collectivism and individualism. He is for a mixed economy, or the present-day fluid type of capitalism, as the best system for assuring the rights of Christian in-

dividualism in an age of totalitarianism, and as the most promising for achieving the reforms suggested by Christianity.

Bennett is a Christian reformer, not a revolutionist, and a distinctive feature of his reform is that it is to be guided by the scientific analysis of economics and the technical knowledge of those familiar with economic facts. He goes beyond this announced purpose and shows ready acceptance of not only the analysis of economists but also their policy recommendations. Bennett's stand on the issues of progressive taxation, labor relations and social security places him in the political spectrum to the left of the public he is aiming at, the clergy and laity of American Protestant churches. Nevertheless, his writing should gain ready acceptance, for it is a popular presentation in the best sense of that phrase, and a solid contribution to the important work of advancing public understanding of economics and economists.

Bennett may be criticized for neglecting some of the fundamental questions raised by other writers in this field. For example, Is the predominant philosophy of the day compatible with Christian philosophy? Bennett draws heavily upon economists, who reflect the predominant philosophy of their times, and their thought may be, at least in part, antithetical to Christian philosophy. Coleridge, one of the earliest students of the relations between religion and economics, stressed the point that "the articles of our Church, and the true principles of government and social order, will never be effectually and consistently maintained against their antagonists till the champions have themselves ceased to worship the same Baal with their enemies." By Baal he meant the mechanic, Lockean philosophy that was predominant in his age.

A second question often raised is: Do church and society face a fundamental and novel crisis today? This concern is expressed, for example, by Emmanuel Cardinal Suhard in *Growth Or Decline?* and by Wilhelm Röpke in *The Social Crisis of Our Time*. If we do face such a crisis, economic analysis of itself will not be a reliable device for detecting it or for suggesting all the solutions needed.

WILLIAM F. KENNEDY

*Santa Barbara College*

*Outline of an Economic Theory.* By BENJAMIN H. J. EIRIKSSON. (Reykjavik, Iceland: Helgafell Publishing Co. 1954. Pp. 493.)

This book, which is the product of a Harvard doctoral dissertation inspired by the late Professor Schumpeter, attempts to reformulate general equilibrium theory. According to Eiriksson, Walras' system is only capable of treating the value and pricing of services because Walras had an unsatisfactory capital theory. But the serious problems of interest, money and business cycles spring from the valuation of "agents." Contemporary monetary and employment theory merely provide "makeshift solutions" for these problems. The road to a really integrated general economic theory, in Eiriksson's view, is by way of a satisfactory theory of capital and interest which can be blended with the Walrasian system. The major portion of the book is devoted to a new conceptual apparatus for handling the problems of capital, interest, and money.

Eiriksson's capital theory is essentially Austrian, but with a novel twist. In

the Austrian tradition, capital ("durable agents") is a means of "storing" the services of "original agents." The services of capital goods are substitutes for the services of original agents. According to Eiriksson, the essence of capital is this process of storing and substitution. In the orthodox Austrian capital theory, however, there are only two "original agents"—land and labor. But the capitalists nevertheless perform a "function" in production by means of their "saving" or "waiting" which makes the original agents more productive via the well-known roundabout methods of production. The reward for the "capitalists" is the famous "agio," as illustrated by the examples of growing trees and aging wine. It is with respect to this particular "functional" interpretation that Eiriksson departs from his masters.

Instead of two "original agents" (factors of production), Eiriksson introduces a third: money. Money is like the original agents of "land" and "labor" since its value does not depreciate with use. (Nondepreciability is the criterion for classifying an "original agent." One might raise the question, however, whether labor and especially land are in fact nondepreciable.) Money also makes the storing of original services (the creation of capital goods) possible. Money performs a "function" coordinate with traditional "land" and "labor." On this basis, Eiriksson attributes the gain on the accounts of capital agents to money as an original agent. The "agio" theory of interest therefore becomes a "service theory of interest."

With this apparatus, Eiriksson commences his analysis, which is overwhelmingly the application of the equalization principle of asset yields under various assumptions. There is a price level of services (flows) and a price level of agents (stocks). The "general rate of return" appears as a derivative from the respective stock and flow values. The "interest rate" is the rate of return on money as an "original agent." In equilibrium the rates of return on different agents, the "general rate of return" and the "interest rate" are all equal. Difficulties arise (which recall Hayek's business cycle theory) when the "general rate" does not equal the "interest rate" either because the "interest rate" is administered or because the "general rate" may not find its "natural" level due to inflexibility of service or agent prices. Also, problems arise when people "prefer" the "agent" money to other "agents" or services. Indeed, all problems appear as types of "disequilibrium" which result from either inadequate price flexibility, or "institutional rigidities," or an insufficient supply of money.

The attempted scope of this book is so vast that it is difficult to criticize it. One may complain that he does not like the concepts. But one man's concepts may be just as good as another's—it is often a matter of taste. Eiriksson, however, does spend an inordinate amount of time laboring his own notions; and the reader has an enormous job trying to keep straight just how Eiriksson's use of a term differs from someone else's. Moreover, one may not always appreciate Eiriksson's efforts to show how people like Keynes and Wicksell were "wrong" or treated only "special cases" simply because they did not use the same apparatus he does. Yet despite its apparant vastness of scope, there is nowhere in this book a clear statement of the forces that determine "the general rate of return." The best Eiriksson presents on interest rate determination is the proposition that the rate is determined by "preferences" for "agents"

vs. "services." One will look in vain for an evaluation of the relative merits of time preference, productivity, and liquidity preference as determinants of the rate of return. In general, this book is long on concepts and short on fruitful behavior propositions. Everything is explained in terms of "preferences." When positive behavior propositions appear, they are often questionable. For example, we are led to believe that higher rates of return on capital and higher wages always encourage more saving and a larger supply of labor. It is not too safe to base a "general" theory on propositions such as these.

To those who seek a grand theoretical system and who find the Austrian capital theory tradition congenial, this book will prove challenging. For those who share Frank Knight's skepticism of Austrian capital theory (which Eiriksson incidentally ignores) and who also feel that theory should be simple rather than metaphysical, this book will be frustrating. In fairness to Eiriksson, it should be stated that the present reviewer falls into the latter category.

J. A. STOCKFISCH

*The University of Wisconsin*

*The Multiplier Theory.* By HUGO HEGELAND. (Lund, Sweden: C.W.K. Gleerup. 1954. Pp. x, 261.)

Dr. Hegeland's book is Volume IX in the Lund Social Science Studies, a series by writers connected with the Institutes for Social Sciences, Lund University. It is published in English and has, we are told in the preface, a primary purpose of presenting "a comprehensive analysis of the methodological problems involved in the multiplier theory and in its application to actual processes of income development." It furnishes us with an excellent review and discussion of much of the literature about multiplier theory and, in addition, traces the earliest notions of multiplicative effects as they were discussed before the publication of the 1931 Kahn article. References are made to the writings of Bagehot (1873), N. A. J. L. Johannsen (1903 and 1913), Fr. Johannsen (1928), and other predecessors of Kahn. However, the bulk of the book is concerned with the Keynesian multiplier and the criticisms, adaptations, and applications of the multiplier principle that resulted from the publication of *The General Theory*: that is, the multiplier of income with respect to investment, derived from a consumption function in which consumption is a function of income.

The chapters on lags and leakages provide a generally adequate survey of these discussions and at the same time make clear the differences between the two. There is a useful section on the relation, or lack of relation, between the multiplier and the income velocity of money. Also, brief sections furnish a discussion of the spatial and matrix multiplier concepts and of the possible uses which may be made of these tools.

Unfortunately, the avowed aim of the book seems to lead Hegeland astray. It is his view that the methodological problem in multiplier theory is that the theory is based on faulty assumptions. In his conclusion, he notes that the logical truth of the multiplier theory is self-evident; he does not wish to appear to deny the formal validity of the theory. It is the application of the theory to policy problems that is his concern, and it is here that he feels that we must

question the assumptions. Since it is the empirical truth rather than the logical truth of the proposition that is in question, one would expect that more effort would be devoted to examining the applications of the theory. At one point, Hegeland recognizes the need for such investigation but states that it is beyond the scope of his book. There is, therefore, only a brief section in the concluding chapter in which the results of the application of the multiplier theory in Germany, the United States and Sweden are discussed. The conclusion reached is that only one clear case could be found in which multiplicative effects on income resulted from a period of deficit spending. There is an overabundance of assertions to the effect that confusion exists in the theory between mechanical and real relationships. Part of this problem, he states, arises because of the absence of expectational influences in the original assumptions.

In a footnote comment, Hegeland further asserts that "all multipliers are mere definitions" (p. 158, n. 1). Since he has previously shown that the multiplier theory under discussion is derived from a consumption function which is a behavior relation, it is difficult to reconcile these two points of view. No further explanation of this statement is made except for a reference to Somers who considered the instantaneous multiplier to be a definition. If it were true that all multipliers were simply definitions, there would be no point in discussing either the logic of the theory or the nature of the assumptions. There appears to be little question that the multiplier theory can be stated in the form of a verifiable proposition. In fact, Hegeland has done this.

There are excellent discussions of the political problems concerned with the question of deficit spending and of the statistical and empirical problems involved in concepts such as a marginal propensity to consume for the society as a whole. Had there been more attention centered on the problem of empirical verification and less on the question of the validity of the assumptions, this book would have been a more valuable contribution than it is. It can be recommended highly for its survey, discussion, and synthesis of a large segment of the literature in this area.

ALLAN H. MELTZER

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*Wirtschaftliche Entwicklung und soziale Ordnung.* Edited by E. LAGLER and J. MESSNER. (Wien: Verlag Herold, 1952. Pp. 456. \$6.00.)

In this work more than thirty economists and social scientists, almost all from Germany and Austria, have joined hands to honor the late Ferdinand Degenfeld-Schonburg of the University of Vienna on the occasion of his seventieth birthday. Most of the articles are written in German, only a few in English and French. The editors were not well advised in choosing a title which is too vague to convey a definite meaning and yet too narrow to do justice to the content of the volume.

Because there is no common denominator among the articles, this review will be confined to a survey of some areas of concentration. There is, for instance, a group of important articles on recent trends in economic and social policies. Most of these are of a highly general character, dealing with such topics as

centralization and decentralization in economic policies (Wilhelm Röpke), problems of the world economy (Adolph Weber), international monetary policies (Joseph Dobretsberger), and social policies (Otto v. Zwiédineck-Südenhorst). Heinrich Niehaus tries to assess the prospects of an agrarian union in Europe. Alfred Müller-Armack, chief adviser to the German Minister of Economics, Ludwig Erhard, interprets the character of the "social market economy," credited with the surprising resurgence of Western Germany after the currency reform of 1948. Somewhat unrelated to this group and the rest of the symposium is a series of essays dealing with the interrelations of law, economics and social organization (Ludwig Adamovich, Alfred Verdross, Hans Schima, and Hans Schmitz).

The overwhelming majority of the other contributions remains within the conventional precincts of economic theory. There are, of course, some departures into the history of economic thought, as in an essay on natural law and economic liberalism (Goetz Briefs, Washington) and a sketch of the earlier stages of economic reasoning (Bernard Pfister). The variety of the other papers is striking. They deal, for instance, with currency depreciation and the terms of trade (Gottfried Haberler, Harvard University), psychological roots of economic dynamics and their significance for value theory (François Perroux, Paris), types of market organization (Alexander Mahr), collective wants (Wilhelm Weber), the public sector of the economy (Anton Tautscher), taxation and socialism (Richard Kerschagl), and welfare economics (Johannes Messner).

While, in the compass of this review, it would be presumptuous to try to convey an impression of the wealth of ideas contained in these papers, an exception should be made on behalf of a defiant article on Keynes, contributed by Hans Mayer of the University of Vienna, generally regarded as the present head of the Austrian School. Both smaller and larger weapons are used in Mayer's frontal attack. Let us first mention his objection to the definition of the labor unit by which, according to Keynes, employment is to be measured. The conversion of an hour of special labor into hours of ordinary labor appears to Mayer not only as a naïve oversimplification, but also as a resurrection of a long-since disproved part of the Marxian labor value theory. In the same context he questions the reality of the alleged concentration of the workers on nominal wages. Although Keynes did not take pains to support his argument by empirical data, Mayer refers to the interest of labor unions in adjusting their wages to index numbers as indicating the opposite attitude. Keynes' basic contention that underemployment may be coincidental with a stage of equilibrium is rejected as too general. It could never happen if wages were flexible. The "fundamental psychological law"—Mayer believes—was neither fundamental nor even a general psychological law. Its validity depended upon the level of incomes and the structure of income distribution. The incomes should considerably exceed the minimum of existence. Of another related element of the Keynesian model, Mayer suggests that it was inconsistent with an essential principle of the *General Theory*. How could the consumption function be assumed to be stable in the short run if, in general, all economic quantities varied with a change in income? Consequently the consumption function should

not be regarded as a pillar of the Keynesian system. It was only the "illusion of a pillar" (p. 48). With the same firmness other Keynesian positions are challenged, for instance, the concentration on full employment instead of on the maximization of satisfaction, the scant attention given to dynamic factors, the predilection for monetary analysis, and, of course, the predominance of the aggregative approach.

In Mayer's view those deficiencies loom too large to be offset by some valuable clarifications of more special problems, particularly in the fields of money and credit. Nor should Keynes be praised as the originator of revolutionary notions. The *General Theory* was not a system; it was not even a theory in the customary sense. Thus Mayer cheerfully subscribes to Leontief's cynical remark that Keynes "seemed to press . . . for reconstruction of the whole foundation in order to mend a leaky roof."

FRITZ KARL MANN

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*Value, Capital and Rent.* By K. WICKSELL. Translated by S. H. FROWEIN. (New York: Rinehart. London: G. Allen and Unwin Ltd. 1954. Pp. 180. \$3.50.)

The recent appearance of this translation of Wicksell's first book, *Über Wert, Kapital und Rente* (Jena, 1893), is both timely and long overdue. Its timeliness arises from the fact that it contains a penetrating evaluation of Leon Walras' *Elements of Pure Economics*,<sup>1</sup> which also first became available in English a short time ago in Jaffé's remarkable translation. The early exhaustion of the reprinted German edition of Wicksell's work shortly after it was issued in 1933 by the London School of Economics is, however, clear proof that there has been an unfilled demand for the present translation for many years.

To scholars and specialists, *Value, Capital and Rent* is of interest chiefly from the standpoint of history of economic thought and more particularly because of the contribution Wicksell made to the marginal productivity theory of distribution. For one thing, his treatment of the role of time in the theories of value and of distribution was original and unique for its period. Further, it is notable that in this work he was the first theoretician to achieve a synthesis between the Walrasian and the Austrian types of economic analysis.

For contemporary students this treatise is likely to be of considerable pedagogical value as an introduction to comparative static equilibrium analysis. It should prove especially instructive to the theoretically inclined, for here they are given more than just a glimpse, at the level of the elementary calculus,

<sup>1</sup> Wicksell's early criticism of Walras, particularly of his concept of capital as propounded in the second edition of *Elements of Pure Economics*, is expressed in *Value, Capital and Rent*, pp. 18-21, pp 92 ff, and pp. 164 ff. In his later writings Wicksell modified his position on the Walrasian theory of interest and capital. On this point see particularly pages 236-38 and pages 225-27 in his article "Professor Cassel's System of Economics," which is appended to the English translation of Wicksell's *Lectures on Political Economy*, Vol. I (New York, 1934). For comments on some of Wicksell's criticisms of Walras, see Jaffé's "Translator's Notes" in Leon Walras, *Elements of Pure Economics* (Homewood, Ill., 1954), pp. 510 ff.

of how effectively mathematics can be applied in economic analysis. As a painless introduction to "mathematical economics," this work has, so far as this reviewer is aware, no equal in the English language, with the possible exception of E. H. Phelps Brown's *The Framework of the Pricing System*. The latter, however, deals with equilibrium analysis in a less complete manner than does Wicksell and further, Phelps Brown reduces, not always without difficulty, all demonstrations to arithmetic.

This translation by Frowein is, on the whole, excellent and true both to the spirit and the letter of the original. However, it is unfortunate that he interpreted the word *Rente* in the title as "Rent" instead of as "Interest." The German word *Rente* has no direct equivalent in English usage, except possibly something as clumsy as "property income." On the other hand, our term "rent" is too closely associated with the Ricardian concept of differential land rent to serve happily in the title of a work which devotes far less attention to the theory of rent than it does to the theory of interest. For the foregoing reasons this reviewer believes a more representative title for the translation would have been *Value, Capital and Interest*.

Shackle's "Foreword" to the English version of this work attempts to establish a direct link between the interest theory expressed in *Value, Capital and Rent* and the cumulative process analysis of Wicksell's later monetary writings, as well as a direct connection between the latter and the monetary analyses of Keynes and of Hayek, which are treated as complementary to one another. This probably stretches one or more points too far. At least in so far as Wicksell was concerned, his theory of capital and interest underwent several changes over the years. The link between the interest theory of the present work and the analysis of variations in the natural rate versus the market rate of interest in *Interest and Prices* (New York, 1936) and in his *Lectures on Political Economy*, Vol. II (New York, 1935) is, to say the least, not very direct. This is due to the fact that he gradually abandoned the period of production concept of capital featured here for other and more fruitful conceptions.<sup>2</sup> As it is, Shackle's essay would probably be a more appropriate foreword for Wicksell's *Interest and Prices*, for which it was not intended, than it is for *Value, Capital and Rent*.

CARL G. UHR

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### **Economic History: National Economies; Economic Development**

*From Mine to Market: The History of Coal Transportation on the Norfolk and Western Railway.* By JOSEPH T. LAMBIE. (New York: New York University Press. 1954. Pp. xviii, 380. \$6.00.)

This interesting volume, the third in the Business History Series of New York University, is a very creditable job. In fifteen well-written chapters Professor Lambie has traced the development of one of the important

<sup>2</sup> Concerning the transformation of Wicksell's capital analysis, see, for instance, the present writer's article, "Knut Wicksell—A Centennial Evaluation," *Am. Econ. Rev.*, Dec. 1951, XLI, 844-48, 850-52, 856-57.

Pocahontas railroads from its origins in the mid-nineteenth century to the present. The major emphasis is on the important events of the late nineteenth and twentieth centuries. This is, however, no noneconomic chronicle of the glamour and achievements of railroading, nor is it a glorification of the company and its management. Rather economists will here find a sober and critical discussion of the economics of railway policy.

Lambie devotes major attention to those matters which are of major concern to transportation economists, such as rate structures, pooling, the relations between coal operators and carriers, rate warfare, the problem of the allocation of the car supply among mines, differentials in the rates from the coal fields to the various ports, and the maneuvers of the major coal carriers, the Norfolk & Western, the Chesapeake & Ohio, the Virginian, and the Pennsylvania to secure profitable coal traffic. He has shown a sure grasp of the major factors in the economic survival of the railroad.

The author has made extensive use of the archives of the railway, but he has carefully balanced this material with that from the many Interstate Commerce Commission cases on the coal trade and the coal rate adjustments as well as other sources on the tidewater coal business generally. The result is a balanced and useful discussion of the rise of this important branch of American traffic. Economic historians will also find that the author has filled a number of gaps in the economic history of the East, while the new breed of specialist, the business historian, will find much of interest with respect to business organization and policy.

JOHN G. B. HUTCHINS

*Cornell University*

*Soviet National Income and Product, 1940-1948.* By ABRAM BERGSON and HANS HEYMANN, JR. A Research Study by the Rand Corporation. (New York: Columbia University Press. 1954. Pp. xii, 249. \$5.00.)

*Soviet National Income and Product in 1928.* By OLEG HOEFFDING. A Research Study by the Rand Corporation. (New York: Columbia University Press. 1954. Pp. 156. \$3.75.)

These two books follow the methodology established by Bergson in *Soviet National Income and Product in 1937* (Columbia University Press, 1953). Altogether Bergson and his associates have now presented comparable cross-sectional national income accounts for the Soviet Union for five important years: the eve of the Plan Era; 1937, the terminal year of the second five-year plan and a turning point in many respects; the last full prewar year, colored by intense military preparations; 1944, the peak of the war years; and 1948, a year witnessing considerable progress in reconstruction and in the "normalization" of economic life after the war. Thus the trilogy is of the greatest significance for our understanding of the structure of the Soviet economy at various crucial points in its development, and of changes therein over time. The authors have not (as yet) deflated their results for the intervening price movements.

The essence of Bergson's methodology is the conversion of actual ruble values, in themselves of doubtful analytical meaningfulness, to "adjusted

ruble" values in conformance with a theoretical "Adjusted Factor Cost Standard" (AFCS), so as to permit the measurement of national product and its components in terms of "real" resource cost. The authors display much insight, statistical skill, and theoretical acumen to this end. But the inadequacies of the underlying data force them to restrict factor costs almost entirely to the return to labor services and the accounting profits of enterprises (chiefly state-owned), and largely to omit interest and economic rent. Moreover, the rationale of treating the accounting profits as a "real" factor cost is—as the authors agree—a debatable one. In a side calculation, Bergson and Heymann attempt to appraise the probable effects of these deviations from the AFCS on the shares of defense and investment in total outlay, 1937-1948 (pp. 75 ff.). The resulting corrections are at times appreciable, though not major.

Even such a large and "simple" factor return as the earnings of hired labor falls short of the requirements of the AFCS upon closer scrutiny. The chief theoretical difficulty here, as Bergson and Heymann point out, is the intrusion of stringent manpower controls since 1940, throwing doubt on the correspondence between wage and salary differentials and differences in "real" costs. Obviously, the authors can do no more than explain the difficulty and try to live with it.

The trilogy bears eloquent witness to the drying up of quantitative information on the Soviet economy over the twenty-year span embraced by it. Hoeffding still has the benefit of voluminous and high-quality statistics for the late 'twenties, and at times is even embarrassed by the riches of his sources. This ample fountain becomes a trickle by 1937 and 1940, and all but gives out for 1944 and 1948. To compile their accounts for the last two years, Bergson and Heymann can rely on only scattered and very fragmentary data, and thus have to resort continually to inter- and extrapolation, indirect evidence, bold estimation, and outright "educated guessing." The result for these years at least in part depends on their *Gestalt*-like perception of the Soviet economy, and to this extent will undoubtedly diverge in detail, if not always in major outline, from the images obtained by other students in the field. But considering that they take pains to spell out their guesses and assumptions, the present reviewer finds in this not so much a cause for criticism as an occasion for commending the authors' courage.

The conceptual and statistical handicaps are compounded perhaps most seriously in the valuation of consumption in kind, chiefly food consumed within the same peasant household or the same collective farm. This the authors express at current average realized farm prices, except for 1944, when they use 1940 average prices. Perhaps indicative of the inherent difficulty of the problem is that Bergson's 1937 study has already been criticized for attributing both too high and too low a value to this component. In this regard one must be careful to eschew criteria that are extraneous to his approach. The AFCS aims only at making ruble values express "real" costs; hence, factors of comparable productivity ought to receive approximately the same valuation. The AFCS does *not* attempt to express by means of ruble values the relative standards of living of owners of the factors, as some of Bergson's critics seem to have implicitly assumed. Nevertheless, valuation of food consumption

in kind at average realized farm prices may not be the procedure most consistent with the AFCS, and, as this reviewer has suggested elsewhere, may have led Bergson to understate the contribution of peasant producers in 1937 (and hence also the relative shares of agriculture and of consumption in the respective totals), if the earning rate of comparable *wage* labor is taken as a yardstick. On the other hand, this understatement (if any) may be much smaller for 1948, not because peasants fared better relative to urban workers in that year than in 1937—they fared much worse—but because agricultural *wage* labor also seems to have earned relatively very poorly in postwar years, judging by some scattered indications in Soviet literature. It should be noted that the authors are very much concerned with the valuation of returns to peasant labor, and both Bergson-Heymann (pp. 58 ff. and 78) and Hoeffding (pp. 63 ff.) offer interesting alternative computations.

The statistical results of these studies will undoubtedly provide food for thought to economists and other social scientists for many years to come. Consider the finding that the rate of gross investment out of gross national product (in terms of “adjusted rubles”) was already as high (23 per cent) on the eve of the first five-year plan as in 1937, and almost as high as in 1948. (If some of the alternative computations are accepted, more of an upward trend is indicated.) But the consumption of households fell, from 66 per cent in 1928, to 56 per cent in 1937, and to 45 per cent in 1948. The decline in the share of consumption was almost entirely absorbed by rising shares of defense, security forces, and communal services. At this juncture the reader is welcome to speculate about the impact of political events—past, present, and prospective—on the course of Soviet economic development.

Another interesting finding is that even at the very height of the second world war, and at a time of extreme consumer privation, the Soviet Union was able and willing to maintain a rate of gross investment of 13.5 per cent. True, some of it was undoubtedly urgent reconstruction in the liberated areas, and some was greatly expedited by lend-lease and other allied help. Moreover, the figure may contain an upward bias in that much of this construction was undertaken with lavish resort to monetary incentives. Still, the fact bears attention. The trilogy thus abounds in significant and challenging statistical results. The authors offer only a modicum of interpretation of their findings; the rest is up to the reader, and, one hopes, to the authors' own pens in the future. Even so, there is no doubt that the work ranks among the most important contributions to western inquiry into the Soviet economy.

GREGORY GROSSMAN

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*The Prospects for Communist China.* By W. W. ROSTOW in collaboration with RICHARD W. HATCH, FRANK A. KIERNAN, JR., and ALEXANDER ECKSTEIN. (Published jointly by the Technology Press, Boston, and John Wiley & Sons, New York. 1954. Pp. xx, 379. \$5.00).

This is an interpretative essay on the prospects of success or failure of the Communist regime in the China mainland. The book is divided into six parts, dealing successively with (1) the struggle for power up to 1949, (2) the evolu-

tion of Communist policy, (3) the regime and the people in the light of Chinese history and in terms of their current aspirations and reactions, (4) Sino-Soviet relations, (5) the Chinese economy, and (6) the prospects of the regime. Parts 3 and 5 are chiefly the products of the associates, and Rostow is responsible not only for the other four parts but also for the formulation of a unified view in interpretation. While interpretation is by nature subject to controversy, I find that his judgment on the whole is remarkably balanced and sound. Rostow need not feel apologetic about "this intrusion by an outsider into the field of China studies" (p. viii), for the book is as significant to students of Communist China as to those interested in the economic development of underdeveloped countries.

The underlying theme of the book is that the Communist regime is primarily concerned with maximization of its political and military power in China, within the Communist bloc, and *vis-à-vis* the external world. The economic development program, with its emphasis on heavy industry and transport at the expense of agriculture and consumers' welfare, is calculated to be a vital measure to attain this objective; and it is upon the outcome of this program that the prospects of the regime will largely depend. Evaluation of the program appears as Part 5 in the book and is chiefly the work of Alexander Eckstein. The first three years of the Communist regime (1950-52) were a period of general economic recovery, price stabilization, and institutional reorganization, thus preparing the ground for sustained industrialization under the first five-year plan to start in 1953. In view of the Communist propaganda on the rapidity of recovery to the prewar peak during this period, it is interesting to find in the analysis that excepting 1949, a year of acute drought, Chinese agriculture had recovered steadily since 1945 and the regaining of the 1936 level by 1952 was a continuation of the same trend. According to the absolute figures on production given by Chou En Lai of the regime in September 1954, which came too late for the author to use, it is also clear that industrial production in 1949 was not as low as he estimated (p. 239)—*e.g.*, there was a 20 per cent underestimate of coal and electric power production. The institutional reorganization is well presented, though reference should have been made to the important Communist technique whereby private enterprises have been reduced to relying almost entirely upon the orders of state enterprises. It has been officially reported that 86.4 per cent of the production of all the large private firms in eight biggest cities was for state enterprises by the first quarter of 1953.

The target of the plan, if Chou's figures for 1954 are to be trusted (and they seem reasonable), is to bring the industrial sector close to the Soviet 1932 level. This, if attained, would be a significant achievement. The study finds that "this rate of growth would not seem to lie beyond the resource capacities of the Chinese Communist Economy" (p. 277). This conclusion is reached with a number of assumptions, the most important of which are (1) that 12 per cent of the gross national product (a rate estimated for 1952)—and a progressively higher rate as time goes on—is for gross domestic investment in the modern sector, this not including nonmonetary investment in the rural sector, (2) that 20 per cent of the industrial capital formation is derived from

exports mainly of agricultural and mineral products, and (3) that a minimum increase of 13 per cent in agricultural output from 1952 to 1957 is necessary to meet the export requirements and the consumers' demand of a growing population and urbanization, provided (4) there is Soviet economic assistance amounting to \$100 million for the whole period. Shortly after the study was made, there came an official announcement of \$130 million of Soviet economic aid. The key to the success of the plan is then held in agriculture. According to official reports, there was an annual increase of 3 per cent in food grains for 1953 and 1954; nevertheless the value of agricultural production (presumably at 1952 prices) had risen only 1.5 per cent for 1953 when the cultivated area damaged by floods and drought was but one-half as large as the area damaged in 1954. This perhaps explains the sudden rate of agricultural collectivization last year, for which the regime was unexpectedly called upon by these circumstances to realize: the number of producers' cooperatives organized by the end of the year was eleven times the target set for the twelve months at the beginning of the year, now covering about one-tenth of the total number of rural households. Whether agricultural production will be increased by this device remains to be seen. There is at least strong supporting evidence that "the success of Peking's First Five Year Plan is not assured" (p. 218).

On very much the same assumptions the author proceeds to construct an interesting model of Chinese self-sustained economic development from 1952 to 1962. It is found that over the decade the gross national product would be increased by some 37 per cent and per capita income by 8 per cent. However, the statistical information released by the regime in the last quarter of 1954 calls for a radical revision of these estimates.

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*The Economic Development of Japan.* By WILLIAM W. LOCKWOOD. (Princeton: Princeton University Press. 1954. Pp. ix, 603. \$10.00.)

Professor Lockwood's monumental study covers the growth and structural change in Japan's economy from 1868 to 1938. His purposes originally were twofold: (1) "to show the importance to Japan of the international order which her militarists had done so much to destroy, and which would have to be recreated in its essentials if she were again to prosper," and (2) to challenge a prevalent notion "that Japan's economic development since the Meiji Restoration had been confined mainly within the sphere of foreign trade and factory industry, that its benefits had been largely drained away in imperialist wars and zaibatsu profits." As his researches progressed over a decade, however, his task broadened into a study of the whole process of Japan's economic growth from 1868 to 1938.

As a case study in economic development this volume will interest many economists, even though they have no particular concern with Japanese problems. It is the record of one of the most remarkable transformations in economic history. Within the space of three generations Japan emerged from national poverty and seclusion to become one of the leading industrial powers. How was it done? What were the key elements in the transformation? What

are the lessons for nations still on the threshold of economic development?

Primary production (food and raw materials) in Japan increased fourfold between 1873 and 1911. This growth clearly outran the 40 per cent increase in population over the same period. It provided a major share of the savings and tax revenues which financed the industrial and military expansion of the period. Nor did growth cease after 1914. From 1910-14 to 1930-34, there was a 45 per cent increase in food production and a 75 per cent gain in output of raw materials. This compares with a 34 per cent rise in population over the same period. More spectacular was the rise in manufacturing activity. In the quarter-century from 1905-9 to 1930-34, there was a fourfold advance. From the turn of the century, for four decades, manufacturing advanced at an average annual rate of almost 7.5 per cent. Greatest relative gains came during the first world war and at the time of the world depression, in 1931-35.

The best criterion of expansion, however, is the over-all growth in net national product. Based on a comprehensive inquiry by Yuzo Yamada of Hitotsubashi University (*Nihon Kokumin Shotoku Suikei Shiryo*, Tokyo, 1951) Lockwood concludes that the aggregate increase was fourfold from 1885 to 1935. This is an annual rate of nearly 3.3 per cent. According to Yamada's figures real income produced within the country nearly doubled in the quarter-century before 1910. It then doubled again during the first world war and the postwar decade. In the boom of the 'thirties the index rose another 50 per cent. The population of the country rose from 39 million in 1885 to 69 million in 1935. Yamada's figures therefore imply a per capita increase of 150-200 per cent in net output.

Although only part of the aggregate growth in income produced was actually available for raising living standards, the rise in consumption levels was substantial. For example, real wages rose 33 per cent between 1897 and 1914 and 65 per cent over the next quarter of a century. The average industrial worker in Japan was able to buy at least twice as much with his daily earnings in 1937 as he could in 1897.

How did all this come about? Lockwood takes pains to minimize the role of the state. "... a study of the whole process of economic growth in modern Japan leads to the conviction that the real drive and momentum lay in large measure outside the realm of national political ambition and State activity. At most the latter only accelerated a process of industrialization which was latent in the whole conjuncture of forces at work." It was, in Lockwood's view, the unleashing of individual ambition and private enterprise at a very favorable point in history (world trade expanding, prices rising, etc.) that brought about Japan's growth.

He doubts that the process can be repeated again in other Asian countries. "Many of the peoples of Asia are developing a set of social expectations and immediate demands such that they will probably be unwilling to tolerate the great inequalities of private property and income so patiently borne by the Japanese." Furthermore he sees little evidence that the well-to-do class elsewhere in Asia "is disposed so largely as it was in prewar Japan to save and invest." He warns that the lesson of Japan indicates that unless population growth can be held in check, the mere increase in numbers will absorb much of

the gains of development, and "unless political institutions can be created to harness productive power to welfare goals, still more of the gain may be dissipated in war and conquest."

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*Britain and the U.S.A. in the Caribbean: A Comparative Study in Methods of Development.* By MARY PROUDFOOT. (New York: Praeger, 1954. Pp. xxi, 434. \$8.50.)

Comparing the methods employed and the results achieved by Britain and the United States in one area where both have had a certain responsibility for nonself-governing people seems at first an intriguing idea. Perhaps by setting one policy off against the other the strengths or weaknesses of both might appear. It was with this possibility in mind that Mrs. Proudfoot set out on her research. Some six years later she comes to the publication of her findings not at all sure that anything of significance has turned up. The effort has some of the irrelevance of comparing apples and horses.

The British are an old and responsible colonial power, organized to carry the obligations of supervisorship. The United States has wanted to pretend not to possess colonies even though temporarily it might have substantial population and areas attached to itself. The British methods are only suitable to a highly professional bureaucracy with a permanent policy and a fixed governmental setting. They could not possibly be transferred to the United States which is without a professional service, has only a rudimentary governmental office to set over against the mighty Colonial Office in London, and has legislators who only infrequently show a sense of responsibility toward subject peoples—especially when the interests of their constituents are affected.

But academic researchers do not give up easily. Mrs. Proudfoot's is one of the series of Colonial and Comparative Studies so ably edited by Marjorie Perham of Nuffield College, Oxford. And she makes the unlikely comparison yield some worth-while results. These are not so much methods which either nation could borrow wholesale from the other as they are materials for self-examination. In the vast undertaking represented by the transformation of a colonial empire into a commonwealth, Britain cannot afford to overlook any experiment or suggestion. And Americans are reminded, if they do not already know, about a chapter of incredible irresponsibilities which may possibly have lessons for the future. Perhaps, after all, the seemingly incongruous developments may yield some returns of a practical sort.

One thing is certain. The United States will never again willingly undertake to administer a conquered people. And the reasons why this is so, and ought to be so, are amply set forth here in many careful pages. These recite the deficiencies of administration, the whimsicality of policy, and the bursts of expensive generosity which have marked the Puerto Rican and Virgin Islands enterprises. And it is made clear that these go back to the nature of our institutions. Informed students have known all this, of course, for a long time. There have been numerous studies, particularly of Puerto Rico, which could lead to no other conclusion. It may even be that self-examination on the part

of Congressmen may have played a considerable part in the decision to establish the *Estado Libre Asociado* or Commonwealth, which has now existed for six years. It can be taken as a confession of inherent disability. For American students Mrs. Proudfoot's study covers familiar ground.

British students very likely have no more to learn. The researches of the past decade have been numerous and competent. But it is worth while to reinforce them with a pointed reminder concerning mistakes which, in spite of a great bureaucracy, a responsible attitude, and well-known facts, have been profound.

The truth of the matter is—though what lesson there is in it, I should hesitate to conclude—that the two creative and reconstructive ideas which have proved to be valuable have come from the Americans and not from the British. These have been the *Estado Libre Asociado* and the development program, both exemplified in the Puerto Rico of the last decade, the one political, the other economic, and each complementing the other.

The British are persisting in fostering a dominion in the West Indies after the pattern of the others so successfully developed elsewhere. But Jamaica and Trinidad, the two considerable colonies—both in population and resources—are separated by 1200 miles of open sea. Stretching in a north-bending arc, Barbados, the Windwards, the Leewards, and the British Virgins have, among them, so scarce a population, and so little commerce with one another, as to ensure a serious divisiveness. It would surely have been more sensible to have followed the Puerto Rican pattern and set up several associated states. Now it is too late.

The British have had a development program since before the war. It resulted from the recommendations of the Moyne Commission of 1938. From the first, however, it concentrated on welfare rather than productive development. The result was a series of fine educational, health and allied institutions beyond the means of the local governments to support, although the policy was not to outrun local resources. In Puerto Rico a sharply contrasting concentration on productive facilities has set in motion an upward spiral so decisive that the island has become a kind of pilot plant for induced economic development among all underdeveloped peoples.

Mrs. Proudfoot could hardly be expected to draw so sharp a conclusion which perhaps I may exaggerate. As a result, however, of not doing it, she is inhibited in accounting for the rapid rise of well-being in Puerto Rico and the relative slowness of development in the British colonies.

I should not expect her to agree that dominion is an impracticable objective for the West Indies. It is by now an accepted British policy, however reluctantly the more prosperous islands approach union with the less prosperous ones. It must be said, however, that there are hints concerning the value of the Puerto Rican example. There is an unacknowledged resemblance to it in the Jamaican development scheme and, so far as they could, others seem also to have followed. But the inherent difficulties make the same approach inexpedient.

It seems to me important to say that one item of American methodology of utmost significance is not fully uncovered by Mrs. Proudfoot, careful as

her surveys have been. The Puerto Rican success is very largely owed, in my opinion, to the intimate connections between various government departments and the corresponding insular departments which have been possible without centralized intervention. Life-giving technology and grants-in-aid from the federal complex flow through Puerto Rico just as though she was one of the sisterhood of states. This is true in agriculture, health, education, public roads, hospitals—all the various state-aid programs. There is no colonial office through which assistance must be routed—and, it might be said, no State Department either. The one has prevented development in the British dependencies; the other has gone a long way to nullify efforts to assist Central and South American countries.

If there is a "secret" in this matter of induced development I feel certain this is it. But, of course, it is not available for British emulation.

Mrs. Proudfoot's book is in sequence rather with Paul Blanshard's *Democracy and Empire in the Caribbean* (New York, 1946) than with such more specialized studies as H. S. Perloff's *Puerto Rico's Economic Future* (Chicago, 1952) and others of a similar sort. The comparative effort may not be very rewarding, but the information is reliable and the observations on the whole are well considered.

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*Economic Problems of Underdeveloped Countries in Asia.* Edited by B. K. MADAN. (Bombay: Oxford University Press, 1953. Pp. iv, 290. \$3.30.)

This volume presents a sample of professional Asian opinion regarding development policy. Most of the contributors are economists, twelve from South and South East Asia, one from the Middle East, and three Westerners now living and working in the East. Their papers are grouped in three sections, which might be titled: the role of foreign capital and technical assistance (6 chapters); surveys of economic development in particular Asian countries (9 chapters); and proposals for an international development authority (1 chapter).

The purposes of the Indian Council of World Affairs in publishing these articles (which first appeared in *India Quarterly*) were "bringing the facts up to date" and "offering detailed and constructive suggestions on the subject." Since the material upon which the volume is based is now three years old, those chapters which concentrate on the first objective miss their mark. They demonstrate, instead, that attempts to give capsule descriptions of entire national development programs make dry and insignificant reading. Fortunately, however, those authors who have ventured policy suggestions have not confined themselves to "detailed . . . suggestions," but have dealt with some of the larger issues faced in common by most of the underdeveloped countries.

B. K. Madan (India) poses the problem of financing basic development projects which have negligible or low yields with foreign grants or cheap loans. Most of this aid must come from a single country, the United States, which leads to bilateral agreements. However, Madan wants an international agency as intermediary. V.K.R.V. Rao (India) reviews the development of thought in

the United Nations and program proposals to deal with this problem—including his own suggestion for an International Development Authority. Acceptance has grown for the idea (and practice) of intergovernmental capital grants as a supplement to interest-bearing governmental, private, and International Bank loans. But Rao's desire for complete separation of grants from actual or implied political commitments by establishing an international granting agency has gone unfulfilled. Nevertheless, Rao's views make good reading alongside current discussions of United States foreign aid.

Several contributors consider the role of private capital in development. Maurice Zinken (Lever Brothers—India) argues that the developing country must embrace private saving as a virtue, acclaim the entrepreneur, and encourage its citizens to strive for material gain—types of behavior which are notably lacking. D. R. Gadgil (India) believes that too much reliance on private capital formation is exactly what is wrong with India's present approach to development. India's official planners might not agree with Gadgil that they have given top priority to public expenditures on agriculture and transport in order to avoid interference with private business in the industrial sector (pp. 83-84). Other reasons can be given. Nevertheless, Gadgil argues forcefully that the Indian government should use stronger measures to increase the flow of savings into its own hands and employ these resources primarily for the development of basic industry.

There is nothing new in the issues or arguments, but for those who would like to know where some influential Asians stand, the book is worth examining.

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*West African Trade—A Study of Competition, Oligopoly, and Monopoly in a Changing Economy.* By P. T. BAUER. (New York: Cambridge University Press. 1954. Pp. xvi, 450. \$10.00.)

Mr. Bauer's work represents six years of research and writing sponsored by the Colonial Office and financed by Colonial Development and Welfare funds. It is unquestionably an able and comprehensive study of the external trade of the Gold Coast and Nigeria. To this reviewer, its value is unfortunately seriously qualified by the author's polemics against practically all interventionist policy and implicitly against the very notion of development planning.

Separate sections describe and analyze the import and export trades, their monopolistic elements, marketing reform policies, and the important development of statutory marketing of principal export crops. Apparently the original project was to focus upon the activities of the few large firms which have dominated the commerce of West Africa and particularly upon the huge Lever Brothers combine, United Africa Co. Bauer clearly shows the basic oligopolistic and often restrictionist character of the markets, but the greater part of the book, notwithstanding its official sponsorship, is an attack upon government policies.

The work focuses principally upon two matters: the role of administrative restriction in perpetuating concentrated markets, and the role of the statutory marketing boards in their alleged effects upon economic development. West

African commerce tends naturally to monopoly because of the special capital and experience requirements. But government policy has served rather to reinforce this tendency and to inhibit potential competition. In Bauer's eyes, true protection for Africans lies only in the promotion of more competition and the reversal of interventionist policy designed to protect African producers, traders, and consumers. The complex of trade, exchange, and price controls; restrictions upon immigration and European land purchase, and statutory marketing serve only to deter possible new entry and investment.

Bauer apparently fails to appreciate that policy objectives other than the vigorous promotion of European commerce must be considered. The history of such areas fails to show how the economic development and welfare of *Africans* can be expected under such a policy. It is more likely that it would serve to perpetuate the typical colonial patterns of monoculture, economic dependency, and low productivity. It would certainly not help provide the basic *public* investment which is so urgently needed to change the existing economic structure.

In any case, Bauer's prescription has little relevance to the development programs of the present popular and largely self-governing nationalist administrations. These have ambitious public investment schemes under way, to which most of the controls are intimately related. Moreover, there are articulate African trading interests to be reckoned with politically.

His favorite target is statutory marketing, and his criticism is merciless and consistent. The boards originated as wartime improvisations. They were retained as a reform of the chaotic prewar pattern, which was characterized by buying rings, a bewildering array of middlemen and moneylenders, and extensive producer indebtedness. The boards have fixed annual buying prices (of cocoa, palm products, and groundnuts mainly) and have accumulated large reserves. These have neutralized inflationary pressures of high export prices and have enabled a higher rate of public development expenditure. Bauer condemns the boards on the following counts: (1) They cut the link between the peasants and the world market; (2) they exact a high rate of forced saving from the producer in addition to a high tax rate; (3) by bulk-sales agreements they have sometimes realized less than the world price; (4) they inhibit private trading interests; (5) they have not defined stabilization precisely—consequently, price-volume relationships have sometimes *destabilized* incomes; (6) the accumulated reserves inhibit the development of private savings and investment; (7) steeply graduated export duties, made easier by centralized marketing, plus the anti-inflationary accumulated reserves, have made possible large-scale public development investments, the economic yield of which is, in his opinion, typically low; (8) low producer prices have restricted cocoa-planting expansion, driving world prices to record heights.

All but the last point are largely valid but essentially irrelevant. They make sense only on the assumption that domestic price stability, planned public investment, and the rational use of agricultural resources are secondary or even pernicious objectives. But domestic food supply is quite inelastic. It will remain so until fundamental agricultural research can prescribe a secure

substitute for the system of shifting cultivation. If producer prices, especially of cocoa, were not restricted, domestic food prices would skyrocket. Thanks to the boards' policies, this has been avoided, enabling development plans to proceed under more orderly circumstances. The reserves of course are still available for future price maintenance.

As for the effect upon cocoa supplies, Bauer neglects the facts that new plantings take seven years to produce, that there is little more good cocoa land, that much new planting nevertheless has been continually undertaken, and that stability of price is a more crucial incentive. In any case, indiscriminate extension of cocoa acreage could very well be disastrous to a precarious vegetation-soil-rainfall complex. Increased output must be sought in higher yields per acre (the boards finance the needed research), and in disease control and rehabilitation (the high costs of which the boards are meeting). Postwar prices are due to short supply, which has lagged because of disease and because planting was discouraged under the unpopular prewar "free" marketing arrangements. The boards cannot be held responsible for the price explosion.

Upon producer price control depend the development plans, the pending Volta River Project, the maintenance of political stability, and the possibilities of transforming a cocoa economy. Usually when such countries receive export-price windfalls, higher incomes and exchange earnings are frittered away, and disorderly inflation ensues. The countries are then castigated by Western economists as irresponsible. African leaders in the Gold Coast and Nigeria are, however, following another path, avoiding inflation, capturing and plowing back such windfalls into basic investment. For this Bauer accuses them of political opportunism and of exploiting their own voters. His strictures are consistent with familiar "free market" dogma, and consequently the many arguments in favor of the boards and their policies, in their specifically West African context, he dismisses. But these countries have different objectives from those of the unplanned economy of Bauer's dreams.

The boards and their policies have been retained by the new African administrations. They are politically supported by the populace, despite some local grumbling—often based, as in Ashanti, upon other political grounds. Bauer attributes this political acceptance to the demagoguery of politicians appealing to the "political" urban elements. But Prime Minister Nkrumah has twice overwhelmingly swept the rural Gold Coast cocoa areas in free elections.

To Bauer, economic development means whatever investment pattern emerges in the private sector of a "free" economy. But West African development requires enormous integrated investment scarcely imaginable under private auspices. Given the development objective, there are few alternatives to the boards' policies.

These criticisms aside, the book is a masterful organization and exposition of new and important material. The author should be especially commended for the impressive selection of statistical information.

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### Statistics and Econometrics

*Economic Activity Analysis.* Edited by OSKAR MORGENSTERN. (New York: John Wiley & Sons. London: Chapman & Hall, Ltd. 1954. Pp. xviii, 554. \$6.75.)

This volume contains thirteen papers written under the auspices of the Economics Research Project at Princeton University, and financed by the Office of Naval Research. The papers are by J. B. Balderston, C. Bernstein, R. Bott, O. Eckstein, J. P. Mayberry, K. Menger, C. P. Modlin, Jr., O. Morgenstern, G. Rosenbluth, T. M. Whitin, Y. K. Wong, and M. A. Woodbury. The volume as a whole must be classified as mathematical economics, although there is one essay (by Morgenstern) which is not mathematical, and a couple of essays (in Part II) which are not economics.

Part I, with four papers, is concerned with input-output economics. The mathematical level is usually that of most papers in this field, and the papers should be of considerable general interest. The opening paper discusses briefly four models of general economic equilibrium, while the second deals in more detail with the Leontief input-output model. The last two papers are concerned with specific problems in input-output analysis, and will be discussed further below.

Part II, with seven papers, is entitled the "Mathematical Properties of Linear Economic Systems," and employs mathematics well beyond the reach of all but a very few economists. Furthermore, most of the problems considered are not interesting to economists, since they are primarily mathematical. The papers apparently develop new theorems about the mathematical properties of linear systems (although this reviewer is incompetent to assess the mathematical contribution), and these theorems should have implications for their economic properties. The fact that few of these economic properties have been developed does not mean that the work will not be useful to economics. Mathematical understanding of these systems, to which these papers contribute, must necessarily precede economic understanding.

Part III contains two papers. Menger discusses some of the logical implications of the laws of return, and Morgenstern discusses the future of "Experiment and Large Scale Computation in Economics."

The general economist will probably be most interested in Menger's paper on the laws of return. This is an outstanding example of how mathematics (logic) can and should be used in economics, and should be required reading for all graduate students who have taken freshman calculus. Menger notes that economists have made the following three statements about the laws of return: *A.* The output of a given piece of land cannot be increased indefinitely by the application of more labor, but reaches a maximum. *B.* An increase in the quantity of labor applied to a piece of land will increase the output of the land by a smaller percentage than the (per cent) increase in the quantity of labor, provided that the initial quantity of labor is large enough. *C.* A given increase in the quantity of labor applied to a piece of land will result in a smaller increase in output, the larger the initial quantity of labor, again provided that the initial quantity of labor is large enough.

Economists have, however, not been satisfied only to assert each of these propositions as true statements about the real world. They have further asserted: (1) that statements *B* and *C* are equivalent, *i.e.*, that *B* implies *C* and vice versa, and (2) that *A* implies both *B* and *C*. Note that these propositions have nothing to do with the real world: their truth does not depend upon any observable phenomena, but only upon logic. Thus it can be—and Menger demonstrates that it is—that statements (1) and (2) are false even though *A*, *B*, and *C* are all true. The lesson that Menger brings home is that we should use the tools of logic if we want to make logical statements.

Specialists in input-output will probably be most interested in the papers in Part I on the aggregation problem, and the problem of allocating trade and transportation charges in input-output models. The aggregation problem arises because it is not feasible to divide the economy into homogeneous industries. The number of industries in the model must be kept within manageable bounds by aggregating the industries into groups. Balderston and Whitin discuss the relative merits of some possible aggregation procedures, and also seek to measure the magnitude of the error which can be introduced by the aggregation procedure.

Trade and transportation charges can be considered as inputs either to the buying industry or the selling industry. Modlin and Rosenbluth discuss some of the theoretical problems involved, and the assumptions implicit in each method of allocating these charges. They also demonstrate that the results obtained from the model are quite sensitive to this allocation. In an 18-industry model for 1939, they find that the method of allocation changes their results by more than 10 per cent for over half of the industries.

Unfortunately, this volume has some of the defects so common to collections of this kind. The different parts of this volume are not directed at the same audience, or concerned with the same kinds of problems. Furthermore, there are times when the right hand apparently does not know what the left hand is doing. Thus we find in a discussion of the assumptions of the input-output technique on page 51 the familiar statement that the coefficients of production must be assumed stable, and in another paper on page 166 we are told, in passing, that, "of course," this assumption is not made. The reader would benefit if problems of this kind had been thrashed out before publication.

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### National Income and Social Accounting

*Long-Range Economic Projection*. Studies in Income and Wealth, Vol. 16. (Princeton: Princeton University Press, for the National Bureau of Economic Research. 1954. Pp. x, 476. \$9.)

To those who judge the worth of economics by its successful predictions, this collection of eleven essays offers evidence that only modest claims are warranted in the present state of knowledge. Although some predictions are attempted, the major emphasis is on techniques of prediction in the funda-

mental areas of productivity and labor force, as well as for fiscal policy, saving and investment, and also for economic sectors ranging from individual industries and agriculture to foreign trade and regions.

According to Simon Kuznets in the introductory essay, the basic premise underlying any forecast is "an identifiable relation between the future and the past, and a minimum of order in the past that can be translated into some specific pattern for the future." In short, a theoretical model consistent with historical regularities is required. In the absence of such a theory, the forecaster is obliged to extrapolate from historically established relationships with a high degree of uncertainty. Goodness of fit, however, is not a sufficient criterion of the usefulness of a function since a variety of functions each fitting the historical data equally well frequently yield widely different forecasts. William Fellner, who explores long-run projection of private investment, suggests that "the predictive usefulness of a theory depends in a large measure on whether it implies a useful separation of 'formal framework' from 'environment.'" This is so because "in economics true projectibility is at present a utopian goal," *i.e.*, no truly comprehensive model exists. To this extent Kuznets and Fellner are in agreement.

The difference between their respective approaches illustrates an important problem in empirical economics and one that is central to prediction problems. In the absence of a generally accepted all-inclusive theory, what weight should be given to the three ingredients inevitably found in any prediction: observed historical regularities, oversimplified theories and "subjective or quasi-intuitive judgment" as Fellner puts it. In the situation envisaged by Kuznets, where numerous hypotheses compatible with observed regularities yield widely different predictions, we select relations on the basis of incompletely tested theories and quasi-intuitive judgment. Fellner, on the other hand, uses an admittedly oversimplified model based on the acceleration principle to predict investment. He therefore puts the greatest emphasis on his interpretation of the *ceteris paribus* conditions since in his view the historically derived parameters are not to be taken too seriously because the parameters themselves are part of the self-adjusting mechanism of the economic system. Hence, the parameters are variables and the forecaster's main task is to judge how they are likely to vary during the forecast period.

The mixture of ingredients used in any projection depends on how much information exists. If the investigator has few observed regularities to work with, oversimplified theories and intuition must play a large, possibly a predominant role. However much reliance must at present be placed on intuitively based forecasts, it seems clear that scientific forecasting is possible only when more mechanical techniques are used because only with such techniques can we begin to track down sources of prediction error. As long as errors are principally ascribable to incorrect intuition, prediction will remain largely an individual matter, a state of affairs that most economists would not relish.

Several of the essays adopt an approach suggested by Kuznets to be an important technique for improving prediction, namely the selection of as many components "as there are sectors in the economy characterized by different

behavior patterns." Harold Wool's treatment of the labor force devotes much attention to the importance of the age and sex distribution for the labor force. For a ten- to fifteen-year period ahead predictions can be reasonably accurate because the youngest members of the labor force as of the prediction date have been born and mortality rates are quite stable. Many unknown sociological considerations become paramount in predicting fertility so that longer-term forecasts are much more uncertain; for example, whether the postwar "baby boom" becomes permanent, which Wool believes is likely.

The analysis of subaggregates also constitutes the focus of Harold J. Barnett's test of the forecasting prowess of input-output. He contrasts the prediction errors of an input-output model with those of a simple regression model (specific industry output as a linear function of gross national product and time) and two naïve models. Barnett finds that the regression predictions are superior to input-output projections while the other methods are somewhat inferior, though the results are not decisive. The only individual industry analysis, that of Paul Boschan, obtains regression estimates for aggregate steel requirements as a function of industrial output and an interestingly derived measure of industrial capacity. A potentially superior alternative to "building in" a measure of cyclical demand variation to account for changes in final output composition would be to attack the matter more directly through input-output. Another study of subaggregates is that of Walter Isard and Guy Freutel. They review some previous theoretical work in the field of regional analysis and present several novel ideas about regional projections and regional interactions. These studies exemplify a general trend toward the use of relatively small units of observation, primarily in recognition of the fact that the behavioral content of the aggregates becomes less and less apparent, the more all-inclusive are the data.

Two studies with empirical content that operate with more aggregative data are those of Fellner on investment and Rex F. Daly on agriculture. Fellner uses decade averages of total investment to derive marginal capital coefficients. These are assumed to be "normal" and are compared to current marginal capital coefficients and the current rate of growth in output in an attempt to determine whether the rate of capital accumulation is consistent with estimated increases in output and saving. Since investment behavior, either in inventories or fixed capital, or in different industrial sectors is likely to be governed by quite different factors, more accurate results might have been obtained had the author used more complete models relevant to the different sectors. Concerning agriculture, James P. Cavin appraises previous forecasts and Rex F. Daly presents a competent although conventional demand study for agricultural output and makes demand projections for 1975.

In addition to a thoughtful discussion of international account concepts, J. J. Polak sets up an equation system in which the long-run demand for imports of other countries is made a function of U. S. imports, each country's capital imports, its exports and its desired reserves. The exclusion of relative prices seems curious in light of widely held beliefs that while the short-run influence of price changes may be small, price effects are much more likely to be potent in the longer run. It might turn out that for prediction purposes

this approach is superior to a price-oriented theory of international trade, although this possibility has not been firmly established.

A succinct and critical appraisal of existing theories of long-run saving by Mary Smelker challenges the notion of a constant secular saving ratio on the grounds that the statistical evidence is inadequate and that the presumed stability of income size distribution does not stand up to close examination. The remainder of her study has been devoted to observations on how income distribution, age distribution, urbanization, the price level, financial assets and consumer durables might affect saving in the future.

Although projections may provide a very rough guide to policy, Arthur Smithies looks upon the possibilities of long-run projections with a sceptical eye, deploring "the amount of time and effort that is now going into the statistical computations of projections which may have little more validity than attempts to guess the height of the emperor of China." Therefore Smithies has concentrated on the impact of fiscal policy, particularly the influence of income redistribution on productivity.

While the volume makes a definite contribution to the topic of long-range forecasting, it is clear that more questions were asked than were answered. This is perhaps as it should be in a field where so little systematic work exists.

EDWIN KUH

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*Individuals' Saving: Volume and Composition.* By IRWIN FRIEND with the assistance of VITO NATRELLA. (New York: John Wiley & Sons, 1954. Pp. x, 288. \$5.00.)

National income statistics give personal savings as a residual of consumer income and expenditures. The Securities and Exchange Commission gives us the story behind this residual in terms of data on the changes in assets and debts making up savings. The savings concepts used by the two agencies differ in several respects, but this fact is incidental to the important fact that the SEC estimates provide information on the forms assumed by savings. Basic to the SEC series are liquid savings estimates, namely those of cash, savings deposits, insurance and pension reserves, mortgage and consumer debt. Other components of savings, namely home ownership and durable consumer goods, appear in national income statistics.

Estimating processes are described in Part II of the study. It is a story familiar to all economic statisticians, of the adaptation of inadequate source data, collected as by-products of legal and business administration, to a statistical problem of social import. One should not dwell on the inadequacies of the results and the need for better data until he has paid tribute to the ingenuity of the methods. These are imaginative and surprising, as fascinating in their way as studies of marine life in ocean depths, or development of binary-digit calculators.

Consider mortgages, probably the most important category of liquid savings, and one in which the data are better and Friend's methods less subject to criticism than in the others. To estimate this item it has been necessary to assume that: (1) recordings can be estimated correctly from sample volun-

tary reports, (2) repayments can be estimated from data of financial institutions and of government-guaranteed mortgages, (3) individuals and non-corporate business own all mortgages on one- to four-family dwellings other than those held by financial institutions, (4) individuals and noncorporate business own no mortgages on structures other than one- to four-family dwelling units. None of these assumptions is good. In a sense, Friend is challenging economists to do something about such conditions. Fortunately the challenge has not been ignored, since governmental and private economists and Congress itself are giving heed to them.

Although frank discussion of inadequacies of data and comparisons of data bearing on the same categories are better in this volume than in most descriptions of economic statistics, they are not satisfactory. They do not squarely face the problem of setting limits of accuracy. Consider, for example, the comparison of Commerce savings estimates and the SEC estimates. We have an extensive discussion of factors bearing on the comparison but we are not given an appraisal of the accuracy suggested by the results. This reviewer, making his own appraisal, judges that in the postwar period, both the SEC and Commerce estimates are probably correct on an annual basis within about 3 billion dollars on a total that varies between 5 and 25 billion. This is pretty good and argues more favorably for the accuracy of component figures than what we know of the figures themselves. On the other hand, the discrepancies in the two estimates are not serially correlated very closely, so that year-to-year changes may have larger errors than the figures for a year.

A need exists for new language developed out of statistical theory for treating errors in economic statistics. They should be considered as a standard error of an estimating procedure. Suppose category  $A$  can be judged to be correct within a certain limit  $a$ , with high probability, and likewise  $B$  and  $b$ . The error of  $A$  plus  $B$  can be intelligently discussed in terms of  $a$  and  $b$  and of the correlation of the two errors. Following naturally also is a concept of the errors of changes in time, that is of  $A(t) + B(t) - A(t-1) - B(t-1)$ . Any other language is awkward and misleading. Without the proper language error discussions must be mealymouthed.

Besides the statistical story, the volume gives us a summary of other findings on savings, and a somewhat cursory economic interpretation of the significance of personal savings. The author believes that investment-savings relationships are important in the explanation of movement in business conditions, stating his views in a Swedish-oriented manner. Had he stuck to this flexible expectational approach, he might have fared better than he did, but he apparently felt obligated to go farther and to embrace the Keynesian postulate of a stable savings function as a justification for his work. We are told: "The reason for focusing attention on the difference between income and consumption is—the belief that if there is a stable relation representing individuals' spending propensities, it is more likely to be between consumption and income than between total expenditures and income" (p. 13). But it turns out that "The precise nature of the saving or consumption function is not known, and there is still some question whether there is in any meaningful sense a stable saving-income relationship either in the short run or long run" (p. 152). Thus,

the findings are negative as regards the existence of a stable aggregative savings function. Moreover doubts are raised as to the usefulness of the savings concept itself. Perhaps, Friend seems to say, the various components of savings should be studied separately and independent of their status in the savings concept.

If we search for findings less fundamental but still of use as tentative hypotheses, we discover a wealth of them. The evidence is strong, for example, that the secular trend in personal savings propensities out of given incomes is either level or downward. Friend also shows that postwar savings can be well explained by regression equations based on savings out of real income in the prewar world. To be sure such equations have not met the acid test of use in forecasting under severe conditions, though they fared pretty well in 1953. Even if proved wrong, such equations have value, since the process of computation and correction of such equations must in itself broaden our knowledge of forces at work in the economy.

From the results of sample surveys of consumer expenditures, the author points out other tentative findings of interest. One which the reviewer found striking is the high importance in savings of entrepreneurial and management groups who seem to account for two-thirds of all personal saving. Friend emphasizes the probable interconnection of savings and investment behavior of such persons. Although expressing fundamental reservations about the results of sample surveys to date, the author considers the potentiality of the survey method as very great.

In Chapters 6-8 the author interprets historical savings data in broader economic terms. This falls short of the theoretical analysis seemingly promised earlier. He is content with a factual summary that hints at important conclusions. From 1929 to 1933, for example, individuals dropped their savings in securities, houses and consumer durable goods tremendously, but liquidated debt and protected their cash and deposit holdings to a surprising extent. This behavior indicated that persons enjoying sustained incomes were retiring debt and accumulating cash, while those with little or no incomes were powerless to lower their cash position or accumulate debt significantly. This suggests in turn that personal savings behavior would not have stabilized the national income at a low level à la Keynes, but in itself would have carried income right down to zero.

In the late 'thirties, high liquidity performance is the striking fact. We learn: "As compared to a negative or small positive figure in the late 1920's individuals' saving in the form of cash and deposits amounted to more than 55 per cent of the adjusted SEC total for the period from 1935 to 1940, and an even higher proportion of the smaller Commerce total" (p. 105).

If the reviewer understands Friend's postwar story of consumer behavior correctly, personal saving was a stabilizing factor in the main. Consumers seem to have contributed a good deal to the inflation of 1947 and 1948, but in the middle of 1948 helped bring it to an end through accumulations of cash and lowered consumer-credit borrowings. In 1949 high consumption and borrowing stabilized the economy by aiding production, and in 1951 their increased saving again acted to prevent inflation.

This book is primarily a source book of fact and summary of findings on savings in relation to income. Economic theory is used mainly to lend perspective and organization. The work should prove useful and stimulating to all empirically minded economists.

PAUL B. SIMPSON

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*Consumers' Expenditure in the United Kingdom 1900-1919.* By A. R. PREST, assisted by A. A. ADAMS. (New York: Cambridge University Press. 1954. Pp. xiii, 196. \$7.50.)

This is the third volume in the *Studies in the National Income and Expenditure in the United Kingdom* published under the joint auspices of the National Institute of Economic and Social Research in London and the Department of Applied Economics of the University of Cambridge. It complements Stone's estimates for the period 1920-1938, the first part of which was recently published. The present volume, however, contains only basic data and virtually none of the demand analyses which were a prominent feature of Stone's work.

The items for which series are presented range from demersal fish (whatever that may be) to licenses for armorial bearings. Inevitably many of the estimates are based on very little firm information, although the author has made use of a wide variety of original sources. Perhaps the most interesting result which emerges is that from 1900 to 1913 real consumption per capita rose by only 4 per cent; in real food consumption per capita there was even a slight decline.

In an appendix an attempt is made to assess the reliability of the estimates. Though this assessment is itself dubious enough, the fact that it is there at all deserves praise. Another appendix is devoted to a reconciliation of Prest's figures for 1900-19 with Stone's for 1920-38, the principal difficulty being that Southern Ireland was no longer part of the United Kingdom during the second period.

Dr. Prest's bold and laborious calculations have resulted in a standard work of reference which will be consulted by economic historians and statisticians for many years. It is fitting that it should also be a masterpiece of printing.

H. S. HOUTHAKKER

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### Business Fluctuations; Prices

*Business Fluctuations and Forecasting.* By CARL A. DAUTEN. (Cincinnati: South-Western Publishing Co. 1954. Pp. viii, 518. \$5.75.)

This textbook, as its title suggests, undertakes two major objectives, a discussion of the theoretical aspects of business fluctuations and a presentation of various methods of forecasting. The first part of the book is devoted to a study of the nature of business fluctuations. Some attention is given to the trend and seasonal pattern but major attention is focused on the cycle. Regional differences in the trends of various businesses and in their cyclical patterns are also considered.

There is a brief section devoted to the techniques of measuring changes in aggregative economic activity, production and prices. This is followed by a rather lengthy historical description of economic trends and cycles during the period from 1783 to the present.

The theoretical explanation of business fluctuations deals with a limited number of the major types of interpretation with more stress on those elements which are common to various theories than to the differences among them. The purpose of this section is the development of a synthetic approach to be used as the basis for forecasting.

One quarter of the book is devoted to the problems and techniques of forecasting including the projection of trends and various methods of forecasting short-run changes of general business activity. Considerable stress is placed on techniques of forecasting for an industry and for an individual business. The book concludes with a description of the services of various forecasting organizations.

The emphasis of the book is reflected in the fact that the first chapter deals with the role of forecasting in business management and the last chapter describes the services of the more important organizations engaged in publishing forecasts of total economic activity or certain special phases of it. The emphasis is also suggested by the fact that one very brief chapter (20 pages) covers a wide variety of programs for the achievement of economic stability while seven substantial chapters are devoted to forecasting.

The book should find its greatest usefulness in schools of business administration where stress is placed on the significance and the techniques of forecasting or in college courses with this emphasis. It will be of less use as a text in courses where primary stress is placed on theoretical interpretations of economic instability and programs for stability which might follow from such theoretical interpretations.

Each chapter is followed by a thoughtful list of selected readings and a list of questions of both the review and problem type. There is a teachers' manual available to those who use the book as a text.

JAMES F. CUSICK

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### **Money and Banking; Short-Term Credit; Consumer Finance**

*Private Credit and Public Debt.* By ANATOL MURAD. (Washington: Public Affairs Press, 1954. Pp. vi, 195. \$3.75.)

In this book, Murad develops a theory of credit, weaves it together with a theory of economic stagnation arising from a form of oversaving, and concludes that nothing, not even public debt creation, can save the capitalist system from ultimate collapse and socialism.

Part I of the book is an elaborate attempt to develop a theory of the origin and role of credit which relates credit creation to the fact that exchange is indirect; goods and services produced are not bartered directly for goods and services wanted. Credit arises out of such "uncompleted exchanges." This is followed in Part II by an analysis of the relation of credit creation to eco-

conomic growth. Net capital accumulation calls forth credit which will not be extinguished unless the economy contracts. This credit is represented by income-yielding securities, which provide "income without working" to savers. When savings, aimed primarily at obtaining income without working, outrun the desire of businessmen to invest, depression results. Such depression can be prevented only by the issue of more private credit to finance consumption, or by public credit. This situation, Murad contends, is the fundamental reason for the strong upward trend in the American public debt. War, he says, has not been the basic factor.

Part III argues that net capital accumulation must decline and crises must ensue in a capitalistic economy. In Murad's words: "... the portion of output devoted to net capital accumulation may be increasing the beginning of the industrialization process; at some point—the point of diminishing net investment—this proportion must begin to decline because of the ever-increasing demands of the production force on the available labor supply; ultimately these demands must become so great that all the workers are claimed by the production force [output of consumption goods, plus capital replacement], leaving none for the expansion force [capital goods expansion]; at this point, when full industrialization has been reached, net investment must cease entirely" (p. 149). Neither population growth nor technological progress, after an "advanced stage of industrialization" has been reached, provides hope that net investment can draw labor away from utilizing existing productive capacity. Saving continues, however, and only government issue of income-yielding credit can satisfy the demand of savers and postpone the collapse.

But even public spending can only postpone the evil day. Public net investment can no more obtain labor from current production than can private. Murad concludes: "It would seem, therefore, that we are doomed to depression and unemployment—unless we stop saving" (p. 167). The ultimate outcome must be socialism, when saving will cease and public credit can be eliminated.

Murad is to be commended for attempting to merge analysis of the credit system with that of the over-all behavior of the growing economy. But many economists will have grave doubts that he has succeeded.

The basic theory of credit developed in Part I seems to me to add little, if anything, to the more widely accepted understanding of society's credit-debt mechanism (as, for example, summarized in A. G. Hart's *Money, Debt and Economic Activity*), in spite of Murad's diatribe against a somewhat straw-man "traditional exposition of money and credit." And serious questions arise in connection with his basic analysis of the process of growth and crisis in a capitalist society. I found several of his central points unconvincing. For example, he argues (without facing up to the statistics) that war has not been the major factor inducing the present large public debt; that saving in a full-employment economy is primarily (exclusively?) induced by the desire of savers for income-producing securities; and that net investment must cease as industrialization proceeds because all labor will be used up by already accumulated productive facilities.

This is a brief book which makes no pretense at presenting elaborate argu-

ment or extensive statistical evidence, but the reader is surely entitled to careful substantiation of such propositions as these. At no point, as far as I can understand the argument, does Murad explain convincingly why his central "principles of diminishing net investment" must even begin to apply, much less why net investment projects could under no circumstances in a "full industrialization" economy bid resources away from consumption goods activities. The supporting argument consists mainly of hypothetical arithmetical examples plus one brief table (p. 140) citing Kuznets' data on net capital formation in relation to national income from 1869 through 1938. The last fifteen years, which show a substantial upturn in the ratio of net capital formation to national income, are, significantly, omitted.

This book is intended as a treatise for economists and serious laymen. I found its basic thesis unconvincing. But the book is lively and stimulating, and Murad warns against the closed minds of present-day monetary economists. Readers especially interested in the fields of money and income-theory thus may wish to investigate Murad's argument for themselves.

G. L. BACH

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*Banking Systems.* Edited by BENJAMIN H. BECKHART. (New York: Columbia University Press. 1954. Pp. xviii, 934. \$13.50.)

This book is a worthy successor to an earlier volume entitled *Foreign Banking Systems*, edited by Professor Beckhart and the late H. Parker Willis.<sup>1</sup> In view of the fact that developments in the area of money and banking have been so extensive since 1929, Beckhart quite properly decided against revising the earlier work and in favor of making a fresh start. As a result, an outstanding book which will be widely used for a number of years to come has been produced.

It will be of interest to those who are familiar with the earlier book to note that there has been some reshuffling of the countries considered. Specifically, Austria, Belgium, Denmark, Norway and South Africa which were included in *Foreign Banking Systems*, are omitted in the new book; and Brazil, Cuba, India, Mexico, and the United States which now receive attention, were formerly excluded. Other countries covered by the new work are Australia, Canada, England, France, Western Germany, Italy, Japan, the Netherlands, Sweden, Switzerland and Russia.

The commercial and central banking systems, savings institutions, urban and rural mortgage credit institutions, cooperative banks, and government credit agencies of each country are treated in detail; and money markets, where they are of significance, are also described. In addition, each section ends with a brief evaluation of the effectiveness with which the existing banking and credit institutions are meeting the needs of the economy of the country concerned.

The contributing authors are unusually well qualified to describe the monetary and banking systems of the respective countries to which they have been assigned. Most of them at one time or another have held private banking con-

<sup>1</sup> New York, 1929.

nections, and many of them have served in government in a financial capacity.

It would be unfair to single out specific sections of the book for attention unless sufficient space were available to permit a review of all of the contributions. The critical comments below, therefore, have purposely been kept rather general.

The discussions of a few countries include an excessive listing of legal references. These are likely to be of interest only to one who is pursuing a research project in the area, and the researcher could easily gather such references from other sources. The average reader is likely to be distracted rather than helped by details of this nature.

An extensive bibliography is listed at the end of the discussion of each country, but in some cases references to pertinent articles in English are not included. This is particularly regrettable in those instances in which there are articles that complement effectively the discussion developed in the book.

As is inevitable in a work of this type, some contributions were obviously completed a year or two earlier than others. Such chronological unevenness as there is would be significant only for the reader who had occasion to compare postwar developments in several different countries.

In a few instances, an unnecessarily detailed listing of the names of individual banks is included. These names will be foreign to and of little importance for most readers.

Finally, and most important of all, it is regrettable that contributors were not encouraged to devote more attention to the question of the relative effectiveness of monetary policies and controls during the period following the second world war. A modest reduction in the amount of descriptive detail and a somewhat greater emphasis upon policy matters would have made the discussion of the various countries more interesting and useful.

The above comments are not intended in any way to detract from the overall excellence of the book. Indeed this is a work which the teacher of money and banking subjects will consider an indispensable part of his professional library. The price makes questionable the feasibility of requiring students to buy the book, but it will undoubtedly be used very extensively for library reference assignments in both undergraduate and graduate money and banking courses.

Coordinating the work of twenty different writers is a formidable editorial undertaking, and Beckhart is to be congratulated upon the skill with which he has guided the work. *Banking Systems* effectively fills a gap which has existed for some time in the money and banking literature.

WILLIAM P. SNAVELY

*University of Connecticut*

*Monopoly and Competition in Banking.* By DAVID A. ALHADEFF. (Berkeley: University of California Press. 1954. Pp. ix, 254. \$4.50.)

This scholarly monograph represents a new approach to banking. In this study the techniques of economic analysis developed in the field of industrial organization and control for the main purposes of analyzing manufacturing industries are now applied to the banking industry. The main focus is on con-

centration in branch banking in California, but this case-study approach is set in a framework of banking market analysis not only for California but for the country as a whole.

The point of departure is the conclusive evidence of increasing concentration in California banking due to the favorable branch banking laws there. Whereas the four large California branch banks controlled less than 15 per cent of banking deposits in the state in 1920, by 1950 the ratio was nearly two-thirds. Furthermore, two-thirds of these Big Four deposits were concentrated in the Bank of America with over 500 branches, reputedly now the largest bank in the United States.

What are the effects of this increasing concentration on the various banking markets in California? In seeking to answer this question Alhadeff compares the functioning of the Big Four branch banks not with any theoretically ideal competitive market but with indices of performance of unit banks both in California and in the nation as a whole. The variables compared are output, price, costs of production and profits. The last are measured as: (1) net earnings on loans and investments, (2) net earnings on assets, and (3) net earnings on capital. In addition to comparing directly profits and other performance measures for branch and unit banks of different size, the author analyzes the impact of the large branch banks on three categories of banking markets determined by size of borrower.

Concentration in markets, product or banking, can not be determined simply by reference to the number of firms selling in a given market but only after consideration of the cross-elasticities of demand for all possible substitutes; in short, alternative sources of supply. Alhadeff fits his banking statistics into a threefold breakdown of borrower-loan markets by size of borrower so that his three basic categories for measuring the effects of banking concentration are the markets of large borrowers, intermediate-size borrowers, and small borrowers. The author concludes after examining his statistics that "intermediate-size borrowers in California face far more limited alternative sources of supply than in other parts of the country" (p. 53). Small borrowers face monopolistic or oligopolistic markets whether they deal with unit or branch banks and large borrowers can have recourse to the prime loan market which is, in effect, a national competitive market though subject to the revolving price leadership of a few large New York City banks. (This market is largely competitive because it is interrelated with other submarkets of the money market such as the commercial paper market, bankers' acceptances market, etc.)

The alleged superior efficiency of branch banks in comparison with unit banks is examined by the author and found wanting. "For various reasons, branch banks do not earn more than unit banks on their loans and investments, and are only of average profitability in terms of their earnings on assets" (p. 192). Although branch banks are more profitable than the largest unit banks on any criterion of profitability, they are in turn surpassed on all three profits criteria by the penultimate size unit bank which is, in fact, the most profitable category of unit bank.

Much of the data in the book is presented in the form of annual averages

for each of the Big Four California branch banks (Bank of America, Security-First National Bank, American Trust and Anglo-California Bank) for the years 1938-1950 inclusive. And although the distinction is made between a legal area, such as a state, and an economic market area, it turns out that for most purposes the legal area of California is also the appropriate banking market area to consider. In general Alhadeff handles his data with theoretical sophistication and carefully compares his empirical conclusions with those which might have been expected from theoretical reasoning based on pure a priori assumptions.

There are times, of course, when Alhadeff seems to stray from the straight and narrow path, as when he asserts: "As a matter of statistical record, therefore, it would seem to follow that the most characteristic product of commercial banks as such is the provision of business loans" (p. 12). Unless for some reason Alhadeff does not regard the purchase and holding of securities as a characteristic product of commercial banks, this interpretation of the statistics is difficult to understand, since his summary Table 1 shows that business loans proper were only 21.4 per cent of the total loans and investments for member banks as of December 31, 1951. (Total loans were 45 per cent of all loans and investments.) Later, he sets the matter straight by referring to banks as multiproduct firms and by stating: "A bank's output can be divided into two main categories: (1) loans and discounts, and (2) investments" (p. 108).

One may also wonder whether or not the analogy of the banking industry with manufacturing industries is not sometimes strained a bit when one reads: "In banking, total assets are a rough gauge of 'capacity' and the ratio of loans and investments to total assets is analogous for banks to the load factor in hydroelectric plants" (p. 57). Why could not the traditional concept of an excess reserve ratio for member banks be used in discussing output performance of banks?

But these are minor imperfections in a work characterized by careful and objective scholarship. This monograph is, in short, an excellent book which can be heartily recommended as desirable reading for all advanced students of money and banking. Aside from the obvious merit of the substance of the book it makes a significant contribution to the application of economic methodology to the field of banking.

JOHN A. COCHRAN

*University of Illinois*

*Le Banche di Emissione in Italia nel Secolo XIX.* By GIUSEPPE DI NARDI.

Vol. 8 of a collection of economic history and doctrines directed by Prof. Pasquale Jannacone. (Torino: UTET. 1953. Pp. xii, 436. L. 2700.)

di Nardi's book about the banks of issue in Italy during the 19th century does great credit to the author for the care and craftsmanship with which he has undertaken a difficult and laborious task. He has brought together and sifted a large mass of statistical and other evidence, much of it not readily available, bearing on the development of the Italian banks of issue, and he has told the story in a very stimulating and readable form. Interwoven with the reporting of actual developments is a good deal of discussion of contemporary thinking on banking questions as revealed in the minutes of Board of Directors

meetings and the annual reports of the banks, reports of the Parliamentary commissions of inquiry, in the economic literature and in public debates. For the most part the author lets the evidence speak for itself and refrains from judgments and evaluation; where necessary, however, he does point up flaws in the analysis of contemporaries and their misreading of the evidence.

The story of the period is presented in three main parts, which cover respectively the period before the unification of Italy and the establishment of a national currency, the period from 1866 to 1882 during which Italy had forced paper circulation, and the period from 1883 to 1893, which starts with the resumption of specie payments and ends with the banking crisis of 1893.

The book adds a new chapter to the history of central banking in Europe in the 19th century, and throws interesting light on the process of economic unification of Italy which is usually treated very sketchily in the more general books on the economic history of Europe. It also offers some interesting side glances on the working of the 19th century monetary systems, particularly bimetallism, in a country poor in capital, in a precarious balance-of-payments situation, and engaged in heavy military and economic expenditures to achieve and consolidate national unity. Italy furthermore was greatly affected by the major economic and financial events in the more important European countries, while at the same time it could not significantly influence those developments.

It is not surprising, therefore, that the history of the Italian banks of issue should present peculiar features of its own. These include the fight, throughout the period, of the regional banks of issue of the center and south of Italy against encroachment by the National Bank, which had been the bank of issue of the Piedmont and which, as the process of unification proceeded southward, gradually expanded its scope and influence, in part because of its special relation to the government to which it provided financial assistance; the clearing of bank notes among the banks of issue which, because of their different acceptability by the public, raised particular problems and difficulties for the five smaller banks; the fragmentation of the money market and the coexistence of several discount rates in the same area, the reluctance to use changes in the discount rate as a tool of credit control; the composition of the banks' portfolio and their increasing illiquidity because of heavy long-term investments in agriculture and industry and financing of the construction boom; as well as the almost continuous yielding to pressures for credit expansion and the consequent ignoring of those aspects of the domestic and international picture which at times would have suggested a different policy.

The history of the Italian banks of issue does not contribute much that is new to the understanding of the development and functioning of central banking. The contribution of the book lies rather in the light shed on the role of the banks of issue in the process of political and economic unification and development of Italy. Indeed one wishes that the author had put his account of the policies of the banks of issue even more squarely within the political setting of the period and the financial policies pursued by succeeding governments. This would also have brought out more clearly the extent to which government action or inaction and political forces were responsible for the

actual course of development of the banks of issue. The disregard of government regulations, abusive practices, the ability of the banks to pursue policies which were obviously not in the interest of sound economic development of the country or of effective central bank management and the ignoring of repeated if mild warnings of various commissions of inquiry, leading eventually to the crisis of 1893—all these features can be fully understood only in terms of the political climate, the reluctance and inability of the government to take forceful action and the influence of powerful pressures inside and outside of Parliament. As a result the benefits which political unity had brought about were seriously jeopardized in the economic field.

It is therefore difficult to agree with the author that the main theme during this period was the issue of monopoly vs. competition among the banks of issue. Although this was one aspect of the problem and a convenient slogan for the advocates of competition in which to clothe their more immediate, direct and particularistic interests, the argument that the eventual crisis was caused primarily by the excesses of competition is not convincing. As the author himself points out throughout the book, not only was there surprisingly little competition among the banks of issue in many fields, but the government, yielding partly to political pressures, deflected and blunted the impact of competition, slowing down and hampering the inevitable trend toward a unified and more efficient banking system and greater economic integration.

None of these comments is intended to detract from the significance of the author's contribution. The book not only adds an essential dimension to the economic history of 19th century Italy, but also contributes a novel and very interesting chapter to the economic history of Europe. One looks forward with interest to the second volume, promised by the author, which will bring the history of Italian central banking up to 1943.

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*U.S. Regional Organisation*  
*Paris, France*

*Money and Credit*. Vol. 1. By MARINOS E. CONSTANTACATOS. (Athens: "Hestia." 1953. Pp. xviii, 145.)

A merchant of Athens has set out, in this small book, to investigate the causes of price fluctuations in this country from 1800 to 1940. The author feels that all of economics must be rewritten—he is especially unhappy with Keynes—and this book serves as the first of a series of volumes toward this end.

He states that price instability is due almost entirely to deviations of the actual growth of the money supply from its "natural" or "divine" or "optimum" growth. The optimum growth is a function of population growth and the "rotation rate of generations," the latter being based on the average length of human life. The optimum growth rate, upon which the entire argument rests, is defined for the first time two-thirds of the way through the book. Even then, the reader will have to refer to a previous work of the

author for a clear statement of the concept. Nowhere does the author attempt to justify the use of the optimum rate, except to state that it flowed from instinct, and to back this up the reader is referred to the writings of Ralph Waldo Emerson.

If the actual rate exceeds the optimum rate of money-supply growth, prices rise; in the opposite case, they fall. The author calls this the Relative Quantity Theory of Money; it is a crude quantity theory in a growth context. The optimum rate is applied not only to the money supply as a whole but to each of its components: currency outside banks and demand deposits. (Actually, without realizing it, the author uses a series for total deposits, including time and savings deposits). The problem is to find which of the two components of the money supply is primarily responsible for price fluctuations. The solution to this problem, according to the author, tells us whether price instability is caused by central bankers (issuers of currency) or by commercial bankers (issuers of deposits). It turns out that deposits and commercial bankers had a stabilizing influence on the price level, and currency and central bankers had a disturbing influence. This result is attributed to "the intrinsic merits of the free market forces, Adam Smith's famous 'invisible hand.'"

However, it is impossible to disregard the influence of the author's hand in this conclusion. Suppose that in some year currency falls and deposits rise (relative to their optimum growth rates), while prices fall. Who is responsible for the price decline? The answer is the central bankers because they permitted the fall in currency, and by so doing denied sufficient reserves to commercial banks. Now, with everything else the same, suppose that prices rise. Whose responsibility is this? There are several answers, most of them commending or apologizing for the actions of commercial bankers. For example, commercial bankers in this case averted "the danger of a further fall in prices." Or, prices rose, not because deposits rose, but as a result of the Balkan Wars. And who can blame commercial bankers for that!

But it is not necessary to pursue this further. The problem itself is a sham. The two components of the money supply, throughout the period under investigation, have not been neatly split up this way between commercial bankers and "central bankers." One has only to recall national bank notes to realize this. Just as important, the implicit assumption that the amount of currency outside banks is solely at the initiative of the issuer is invalid. A "deficient" supply of currency may simply reflect the public's preference between the two components. Furthermore, the author assumes that the significance of currency outside banks is that it acts as the reserves of commercial banks, and this is not true either. It is difficult to think of a more inappropriate banking series with which to measure commercial-bank reserves than that of currency outside banks.

Aside from the component "problem," the analysis as an explanation of past price behavior is clearly deficient. The author's sole reliance on the transactions equation,  $MV = PT$ , will convince many readers of that. Within this framework, however, the explanation for changes in  $T$  in terms of population growth and the rotation rate of generations is woefully inadequate. Moreover,

changes in  $V$  are never considered. And when, according to the author's own standards, the wrong series for the money supply is used, the analysis becomes hopeless. All that remains intact is the price series.

We next find that the central bankers are not really to blame for price instability; rather it is the gold standard. The relatively slow growth of our monetary gold stock holds back the growth of central-bank liabilities. The latter in turn holds back the growth of commercial-bank deposits, and this results in a deficient money supply. If the central bank attempts to expand its liabilities unduly, the ratio of gold to these liabilities falls and the bank becomes illiquid. If commercial banks attempt to expand their deposits unduly, the ratio of their reserves to deposits falls and they become illiquid. Either way, the monetary system is doomed. The author illustrates this and allied points over and over again with literally hundreds of equations and numerical examples. The upshot usually is that by the year 2055, if not before, the whole system has blown sky high. "My conclusion is that under the existing credit management the outlook is truly ominous."

It may well be true that fifty or one hundred years hence we shall suffer from too little money if we insist on maintaining the present reserve requirements on both commercial banks and the central bank. Money-supply growth calls for the growth of central-bank liabilities (commercial-bank reserves) and, if reserve requirements on the latter are maintained, the digging implied by this may be more than the gold miners of the world will be up to. However, it is one thing to believe this possible, but quite another to argue that it spells catastrophe. If we cannot dig gold rapidly enough, surely we can create sufficient debt for the monetary system to purchase. Rather than worrying about the liquidity of the system, it would seem more important to concentrate on the alternative means of attaining a proper rate of growth of the money supply. If private debt is forthcoming, due to deficits in the private sector and surpluses in the government sector, commercial banks will probably be the purchasers of some of the debt, and this may necessitate a continual lowering of their reserve requirements. If, on the other hand, public debt is forthcoming, the central bank could be the heavy purchaser and at the same time could even raise reserve requirements on commercial banks. There are many combinations between these extremes. Over the long run, it is important to adopt the optimum combination of open-market purchases, treasury asset acquisitions, and reserve requirement changes that will bring about the proper rate of growth of the money supply. The choice of alternatives implies close cooperation between the fiscal and monetary authorities. It would not seem necessary for them to cooperate closely with the gold miners.

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### Public Finance

*The Theory of Fiscal Economics.* By EARL R. ROLPH. (Berkeley and Los Angeles: University of California Press. 1954. Pp. xiv, 310. \$4.50.)

As the title indicates, this is a theoretical book containing little institutional

material. It includes chapters on the nature of government economic activities, social costs of government, government and social accounting, the monetary basis of fiscal theory and seven chapters on tax incidence in the broad sense. The analysis is careful, and the writing style is clear and forceful. Rolph had previously presented in journal articles some of the ideas developed more fully in this book and they have already aroused discussion. The book will doubtless receive a great deal of attention from public finance specialists and seems likely to excite some controversy.

The chapter on social accounting sets forth a well-reasoned argument for treating both public and private interest payments and dividends as transfer payments. Rolph rejects the factor-cost method of valuing national product or income on the grounds that it is uncertain that so-called indirect taxes in fact raise prices and that, in any case, price increases should not be concealed, whatever their cause.

In the chapter on monetary theory Rolph contends that private expenditures depend on stocks or assets rather than on income. This conclusion is supported mainly by the argument that income of any period cannot determine consumption (or investment) of that period since two contemporaneous events must be causally independent of each other. Although most economists would probably now concede that wealth or liquid assets as well as income influence consumption, it seems extreme to discard income altogether as an explanatory factor.

As regards incidence, Rolph sets for himself the praiseworthy objective of rescuing the topic from the status of "a series of footnote thoughts on price theory." The great merit of his treatment is his consistent emphasis on general equilibrium or aggregate demand implications. Perhaps his most startling statement is that excise taxes are not shifted to consumers but instead reduce factor returns. He argues that a general excise tax is deflationary and hence will not raise the price level. Furthermore, it will not change relative outputs or prices because there is no untaxed employment to which factors may go when their money returns are reduced. These points had occasionally been recognized, notably by Harry Gunnison Brown, but Rolph deserves credit for developing them more fully. He goes much further, however, and extends the argument to selective excises. A tax on a particular commodity will cause non-specialized factors to shift from this industry to other employments, and returns in these fields will be forced down. If aggregate money demand and real output remain constant, the tax proceeds will all come out of factor returns and a wide range of industries will be affected. But this reflects only an accounting identity: final expenditures equal factor returns plus excise taxes. It does not necessarily mean that the "burden" or incidence of the excises is on producers. Could we not equally well say that commodity taxes are always paid by buyers since their outlays cover both factor returns and taxes? I am not sure under these circumstances whether it is profitable to discuss whether producers or consumers as a group bear the taxes, but I do feel that Rolph is justified in insisting that the conventional treatment is oversimplified.

The surprising omission is Rolph's failure to consider systematically changes

in relative prices and factor returns. These changes represent tax shifting in the traditional sense and their determination has usually been regarded as the study of incidence. A tax on a particular industry will raise the relative price of its output, unless factors employed by the industry are so highly specialized that they are unable to transfer to other employments; and it will reduce relative factor earnings in the industry, unless the factors are unspecialized and highly mobile. Persons whose tastes run toward consumption of the taxed commodity will be taxed more heavily than other consumers, and owners of specialized factors will be penalized. Surely the beer drinker is right in believing that he is being taxed by the excises on beer and the fur worker in believing that the excise on fur coats depresses his wages. The traditional theory of incidence, which is only an application of price theory, concentrated attention on such changes in relative prices and factor returns. More distant repercussions were neglected, in conformity with the usual partial-equilibrium method. It was not always recognized that this partial-equilibrium theory of incidence, although applicable without serious limitation to small taxes, could not be easily extended to taxes of wide coverage and large yield or to a large group of small taxes considered jointly. Rolph's treatment is a useful corrective for this shortcoming, but in my opinion he goes much too far in discarding traditional incidence theory when he refuses to grant that an increase in relative price of a commodity constitutes "shifting" of a tax to consumers of that commodity.

In discussing import duties, Rolph rejects the proposition laid down by Mill and endorsed to a limited extent by Marshall that these taxes fall partly on foreigners, although he agrees that the duties may turn the terms of trade in favor of the country imposing them. Foreigners do not pay money directly or indirectly into the treasury of the taxing country and hence, according to Rolph, cannot be said to bear the import duties. In the taxing country, Rolph believes that import duties fall on exporters or other producers rather than consumers of imports. Exporters are "taxed" if the duties cause the value of imports (exclusive of tax) to decline, since export receipts cannot in the long run exceed payments for imports. This treatment seems to me to imply too literal an interpretation of the meaning of "taxing the foreigner" and again demonstrates Rolph's refusal to classify a change in relative prices as tax shifting.

Rolph's conclusions regarding the effect of taxes on the incentive to work seem to be close to those that are now rather generally accepted, although the manner of statement may suggest more novelty of substance than I believe actually exists. He accepts the proposition that an income tax lowers the price of leisure and hence, from this point of view, tends to cause people to work less. He also agrees that if a tax reduces the real income of a person it will for this reason give rise to an offsetting tendency to induce more work. He differs from most other writers, however, in stressing the price effect and minimizing the income effect. He argues that for society as a whole taxes serve only to prevent inflation and impose no real burden; the real burden is due to public expenditures. Rolph concedes, however, that taxes may redistribute income and thus have income effects that will partly or wholly offset price effects for certain groups.

In the final chapters, dealing with taxes and investment, Rolph presents a highly sophisticated analysis of tax capitalization. He shows that many discussions are faulty because they are confined to the effect of taxation on expected future yields of assets and take no account of the (usually offsetting) influence on rates of capitalization. I find it quite impossible, however, to accept his conclusion that, under conditions of certainty, taxes will have no effect at all on the rate of investing. This conclusion is based on the flat assertion that people save and invest in order to become wealthy and not to earn a return. Granted that wealth may be desired in itself, surely the possibility of receiving an interest return is an additional motive for saving. Even with the speculative motive for holding cash ruled out, it is hard to see why savers would go to the trouble of investing instead of hoarding if investments offered no return.

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### International Economics

*The Economist in the Twentieth Century: And Other Lectures in Political Economy.* By LIONEL ROBBINS. (New York: St. Martin's Press. 1954. Pp. xi, 225. \$3.50.)

This collection of lectures, delivered in Rio de Janeiro and elsewhere, deals mainly with problems of international readjustment. Professor Robbins here adds his contribution to the recent array of small but distinguished works—by Robertson, Harrod, Hawtrey and other English economists—on the great post-war disequilibrium.

Inflation in the deficit countries is the villain of the piece. The fiscal methods of inflation control, which Keynes outlined in *How to Pay for the War*, are seen to be subject to serious practical limitations. Failure to use more promptly the tools of monetary policy receives the major blame, but there is hardly any discussion of investment compared with consumption as the proper object of retrenchment. The prescription is: stop inflation, correct the exchange rates. There is full confidence in the efficacy of exchange rate changes; statistical estimates of low elasticities are dismissed as worthless. The author strongly favors, however, the International Monetary Fund's "adjustable peg" procedure, with exchange stability the rule rather than the exception. In his view it is a mistake to rely on continually fluctuating exchange rates as an automatic means of equilibration (pp. 97-101). In the use of import restrictions by deficit countries Robbins admits the case for—at least temporary—discrimination against the surplus country (pp. 57, 144). On the question of sterling convertibility his stand is cautious, because reserves are uncomfortably small. He favors a general increase in the price of gold as a way of restoring international liquidity, but regards it as impracticable at present because of U.S. objections. In place of European union, which he considers too narrow a goal to be attainable, he demands nothing less than integration of the Atlantic Community.

Robbins' views will command wide agreement, though naturally, over so vast a range, there are points to which some will take exception. The author is right when he castigates the habit of keeping domestic and external policy

in closed compartments, but to my mind he goes a little too far in treating a high level of employment and equilibrium in the foreign balance as incompatible. Besides, he devotes a whole chapter to criticizing Beveridge's employment proposals without mentioning the 3 per cent ratio which Beveridge proposed as the normal rate of unemployment, a goal that has turned out to be conservative compared with the actual ratio of 2 per cent or less that has prevailed in England since the war. In the classical theory of commercial policy Robbins perceives a disquieting flaw: the case for unilateral free trade is weakened, in his opinion, by the fact that tariff reductions by one country alone will have deflationary effects on that country's domestic economy, whereas with reciprocal reductions losses are counterbalanced by gains (pp. 135-40). The argument seems incomplete, the concession unfortunate. If employment theory reveals a flaw, that theory can also take care of it, by showing the way to realizing the classical assumption of full employment. Especially for a country with a strong balance of payments there is nothing to prevent the offsetting by means of internal expansion of any deflationary pressures arising from unilateral tariff cuts.

Apart from Beveridge, the only living writer cited more than once is Jacob Viner, "that fine economist and true citizen of the world" (p. 167). Views criticized are generally attributed to "some high authorities" or to other nameless—and consequently somewhat shadowy—characters. The author evidently prefers to stand, urbane and dignified, above the fray of current controversy.

The book begins with some very interesting reflections on the growth in the demand for economists and for the teaching of economics in the last fifty years, and ends with an essay on the British war economy, in which British economists, including Robbins himself, played such an outstanding part.

RAGNAR NURKSE

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*An International Comparison of National Products and the Purchasing Power of Currencies.* By MILTON GILBERT and IRVING KRAVIS (Paris: The Organisation for European Economic Co-operation. 1954. Pp. 203. \$3.00.)

This is a prodigious piece of work and an honor to its authors. It is one of the few major pieces of empirical research done in the field since Carrol Wright's work in 1885. Here is a forthright discussion of the methods and problems of comparisons of the gross national products of different countries, plus a detailed showing of the results of one such comparison. The findings will undoubtedly be put to substantial use in international negotiations.

The study is designed to provide "some quantitative measure of the relative economic strength" of member countries of the Organisation for European Economic Co-operation (pp. 13, 64); to "measure the relative economic ability of the five countries to meet the requirements of present-day existence" (p. 18); to assess "the productive ability" of the countries involved (p. 61). Clearly these are all versions of a single purpose. The goal is achieved by binary comparisons of "the flows of final goods and services in the two countries" (p. 63)—namely, the United States and United Kingdom, United States and France, United States and Western Germany, United States and Italy. These

comparisons were made by identifying and pricing "about 250 goods that were comparable in the two countries" (p. 77). The purchasing power equivalents thus computed for a given product—European currency units per U. S. dollar—were weighted together and applied to GNP product class totals—for cereals and cereal products, laundry, dry cleaning, and so on. Data on comparative GNP totals and components, as well as on purchasing power equivalents, are reported for each country both at average European and at United States price weights. The key point of the operation was the direct field collection of prices in the various countries for clothing and shoes, with extensive detailed pricing from unpublished and published official sources for other items. Nearly 80 pages of notes indicate the complexities of the pricing process thus briefly summarized.

Among the major findings of the study are the following:

- A. The usual conversion of national product data by exchange rates (as is done, for example, in assessing contributions to the United Nations and other international organizations) has "severe limitations" (p. 16). It tends to understate the internal purchasing power of the national currencies for the items composing their national output. Thus the output of the 4 European countries totals about 50% of the United States total when converted by purchasing power equivalents in this study as compared with only 40% from United Nations conversions.<sup>1</sup>
- B. The gross product of the United Kingdom in 1950 (valued at a geometric average of United States and United Kingdom relative prices) amounted to 19.2% of the United States total. For France and Germany the totals ran to about 12%, and for Italy, to 8%.
- C. On a per capita basis the United Kingdom's output was 58% as great as the United States'; the French, 48%; the German, 38%; and the Italian, 26%.
- D. Data are shown in extensive detail for GNP valued separately by United States and by foreign prices. The "use of United States price weights produces national product figures that are higher by 20 per cent for the United Kingdom, about 25 per cent for France and Germany, and 40 per cent for Italy, than the estimates derived by means of the European price pattern—the base figure in all cases being the actual United States gross national product" (p. 25). A shift in weights from United States to European produced no change in the product totals for some items (domestic service) and a change of more than 100% for others (vegetables and fruits in Italy).
- E. An astute discussion of the relationship between quantity and market price ratios indicates the impact of relative prices and output. For autos and other consumer durables European prices are relatively high (high purchasing power equivalents) and consumption relatively low (low quantity ratios). For food, differences between European and

<sup>1</sup> United Nations data from United Nations, *National and Per Capita Incomes of Seventy Countries in 1949*, Statistical Papers, Ser. E, No. 1. Gilbert and Kravis give a figure of 35% rather than 40%—which they derive by using actual exchange rates. In practice the United Nations has used an adjusted exchange rate for the United Kingdom.

United States prices are much less and differences between European and United States consumption levels are much smaller.

Which components primarily account for the per capita gross product in the United States being \$674 (or more than 50%) greater than the United Kingdom's and \$1,262 (or more than 200%) greater than Italy's? They are actually few in number. In consumption—clothing and household goods, transportation, and miscellaneous. In investment—producers durables. In government—defense. The GNP differences for food, housing, health, education and recreation account for only a small part of the wide gulf between the United States and European GNP. (They are, of course, substantial on an item comparison basis.) The difference in investment expenditures is perhaps the most striking single difference. However, these investment differences are gross, comparable depreciation data being nonexistent. Net investment differences might be less, given the greater body of capital equipment in the United States.

The major contribution of this study is certain to lie in the wealth of detail on GNP components and purchasing power equivalents in the five countries studied. This detail enables the reader to make his own combination of components, and direct his attention to specific items. It is nevertheless worth speculating how well the report answers the questions which it set out to deal with—(1) What is the "relative economic strength" of these four OEEC partners as compared with that of the United States?; (2) What is the relative ability of these countries "to meet the requirements of present-day existence"?

1. References to "relative economic strength" (p. 1) and to efficiency "from an economic standpoint" (p. 25) are made in the context of "real comparisons of national and per capita products." But just what does a comparison of GNP totals tell us about strength, about efficiency? How many officials who compare these GNP totals will understand that one country will have more "economic strength" than another in proportion as it has a more complex financial system (more checks used, more services of financial intermediaries); higher interest rates (more interest paid); more barratry (more legal services); and more residents who take thought of the morrow (more expense of handling life insurance)?<sup>2</sup>

The root of the difficulty lies in the fact that while all legal marketed products enter into the GNP total, the resources used for certain categories of production are committed to that particular use—and are not available for other uses, such as defense output. Comparisons of the economic strength of members of international organizations must reckon with that distinction: resources used in making \$100 worth of automobiles may be available for making \$100 worth of tanks, but \$100 worth of vaudeville services may be quite unusable for any other purpose.

A second difficulty lies in the fact that GNP measures output rather than potential—though the latter is implied in reference to "productive ability"

<sup>2</sup> It is clear that production, of any item, will require the use of real resources and it is for this reason that any legal marketed item is included in the GNP total—whatever the estimator.

(p. 61). Actual output, of course, is a fact, and perhaps the best single measure of capacity. But these are not identical; and this is particularly important where a country may be undergoing substantial economic or administrative change. Thus comparisons of Italian GNP with that of other countries measure productive ability less well than do comparisons for the United Kingdom. Changes in land tenure may bring marked jumps in productivity and hence in output—as the experience in more than one underdeveloped country has indicated.<sup>3</sup> (Even within the confines of this study, of course, one may note the dramatic gain in the German gross product from 1950 to 1952.)

2. A second stated goal of the OEEC report is to “measure the relative economic ability of the five countries to meet the requirements of present-day existence” (p. 18). There is, of course, a broad similarity between the structure and goals of these five economies. But the contribution of this study is beyond the pointing out of broad differences in GNP. It lies, rather, in the measurement of GNP in dollars, and purchasing power equivalents to three significant places. In such a context one may question whether there is a single set of requirements of the kind which would warrant using the GNP totals and purchasing power equivalents for international negotiation.<sup>4</sup>

An extensive argument has taken place about the contribution of distribution per se to economic welfare. The OEEC report takes the only feasible path and includes such costs in GNP. But it is to be hoped that future editions will segregate an estimate of distributive margin so that users can appreciate how this item contributes to GNP differences. Using the ratio of income originating in trade to gross domestic product as a fair approximation of trade margins, one finds that the trade margin differential between the United States and Italy, for example, contributes as much as does the spread in investment to differences between the two countries’ “comparative economic ability to meet the requirements of present-day existence.”

A further complication is the traditional one that GNP measures only those items which come within reach of the measuring rod of money. Few people would confound the national accounts in a vain attempt at complete inclusion of all nonmonetary items. But equally we must recognize that differences between the United States and these other countries, particularly Italy, are affected by such elements. One of the major differences in consumption, for example, is the “miscellaneous” category, of which a substantial component is interest. Because we “net out” intrasector transactions in computing GNP we recognize a larger share of money lending in the United States (where small-loan companies are incorporated) than we tend to in Italy (where the money lender is more apt to be an individual). Similar considerations apply to consumer expenditures for betting (*pari mutuel* versus small-scale betting)

<sup>3</sup> Cf. Walter Heller in Haskell Wald, ed., *Papers and Proceedings of the Conference on Agricultural Taxation and Economic Development* (Cambridge, Mass., 1954), p. 236. The expansion of the national health program in the United Kingdom led to a 13% rise in medical consultations—a rise which would narrow the United States-United Kingdom GNP differential if medical consultations were priced on an item basis. (Actually the OEEC study simply compared the number of physicians per capita for this component of GNP).

<sup>4</sup> This problem, of course, is equally present when we use exchange rates or international units for deflating national income totals.

for moving (transportation companies versus transactions between individuals) and so on. In sum, we can less accurately speak of these GNP totals measuring "the relative ability of these countries to meet the requirements of present-day existence" than we can say: the United States comes closer to meeting the requirements of present-day existence in the United States through the market mechanism than Italy comes to meeting the requirements of present-day existence in Italy through the market mechanism. The revised statement is somewhat more tautological; it is nevertheless more accurate.

Having considered some of the points which must be borne in mind in using the GNP totals in this report for international comparisons of economic strength and ability to meet present-day needs, we must go somewhat further. For the nature of economic development may well be such that there is a broad correlation between the growth in margins, in services and in the basic investment components which are traditionally the measure of economic power. It may be of interest to those accustomed to use iron and steel output as a simple measure of "economic strength" to compare the results of the OEEC study with the production of crude steel in 1950:

Production of Crude Steel		GNP Valued at Geometric Average of US and European Prices (Gilbert and Kravis)	
United States	100		100
United Kingdom	19		19
France	12		13
Germany	14		12
Italy	3		8

The results are strikingly similar except for Italy.

To judge the validity of the Gilbert-Kravis findings one should have spent at least a hundredth as much time in detailed research as have the authors. Certain comments can nevertheless be ventured.

A. The findings of this study are broadly consistent with certain earlier reports. Thus, for example, the Gilbert-Kravis results indicate that United Kingdom consumption in 1950 was roughly 66% of United States. A small-scale study by Adler and Paige for 1948 indicated a ratio of 63%.<sup>5</sup> And the pioneering study in the deflation of GNP, done by Copeland *et al.* for the Combined Production and Resources Board suggested a ratio somewhat above two-thirds for 1943.<sup>6</sup> United Nations comparisons imply a markedly smaller ratio—but conceptual differences could easily account for the difference.<sup>7</sup> The comparisons made by the U. S. State Department in computing foreign

<sup>5</sup> S. Adler and D. C. Paige, *International Comparisons of Consumption* (Cambridge, 1952). Data are taken from Table 2 of this mimeographed study.

<sup>6</sup> Combined Production and Resources Board, *The Impact of the War on Civilian Consumption in the United Kingdom, the United States and Canada* (Washington, D.C., 1945), pp. 1, 1935.

<sup>7</sup> United Nations, *Retail Price Comparisons for International Salary Determination* (1952), Ser. M, No. 14, pp. 23, 27, 39.

service allowances, the ILO food comparisons, Colin Clark's most recent estimates—these and still other data could usefully be summarized and compared by the authors in a subsequent edition of this study.

B. The basic findings on purchasing power equivalents seem consistent with exchange rates. Thus the United Kingdom exchange rate in 1950 was .357 pounds per dollar—as compared with a purchasing power equivalent for the entire GNP shown here of .288 (U. S. quantities). But the equivalents reported here for clothing and textiles and for producers durables are .316 and .303—or close to the exchange rate, and such items are likely to be representative of major segments of United Kingdom exports. For Germany the exchange rate was 4.20 marks per dollar. The report finds equivalents of 4.26 for coal and 4.42 for producers durables—items which we may take to be reasonably representative of the volume of Germany's coal and metal product exports. One of the major findings of the report is a demonstration that by adding together *all* the items in the national product—food, housing and so on—one arrives at GNP purchasing power equivalents in these four European countries which are markedly better than rates for the particular components which move in foreign trade.

C. The differentials between purchasing power equivalents are sensible for the chief components of the national product. They run in line with the influence which one would expect heavier United States capital investment and greater productivity to exercise. Our relative advantage over other countries is less for those products where labor is the dominant cost, rising steadily as the degree of fabrication increases and greater United States capital investment begins to have its impact.\*

D. What can we say of the results on their own ground—that of internal consistency? Again, the vast bulk of results are reasonably consistent. A baker's dozen, however, are perplexing. The items designated are those where the relationship of one set of equivalents to the other may be quite reasonable to those acquainted with the country and the data but do raise questions on a priori grounds.

Why, for example, should ratios for entertainment and for "other recreation" be so far apart in Germany and Italy—particularly considering that the entertainment ratios were used in deriving the latter?

Since laundry and barber shop services were directly priced in all countries, and since the overwhelming portion of the price in each instance must be average earnings of proprietors, it is hard to understand why the near identity of ratios in the United Kingdom is so totally absent in Germany and Italy. (Domestic service equivalents—though derived by different means—give ratios very similar to those for barber shops, very different from laundries.)

Why should the housing and residential construction ratios in France and

\* Particular points, of course, are out of line. It is puzzling why the United Kingdom's purchasing power equivalent for meat and fish should be relatively so advantageous given the heavy volume of meat it must import. It is somewhat more puzzling why the French should apparently be such inefficient producers of shoes. Perhaps the fact that no data were available for this item from official pricing for the French cost of living index, with reports based only on direct pricing (p. 135), may have been responsible.

Italy be so far apart? Does the lower purchasing power equivalent for housing amount to a startling reflection of rent control?<sup>9</sup>

Both nonalcoholic beverages (tea, coffee) and tobacco are imported products in these countries. Yet while both products are above the consumption average in Germany and Italy one falls much lower in the United Kingdom and the other much lower in France.<sup>10</sup>

The ratios for purchased transportation equipment—essentially automobiles—and for producers durables were virtually identical in France and Italy, but diverged widely in the United Kingdom and Germany. Since motor trucks are included in producers durables (presumably with substantial weight) and since automobiles dominate the former group, the reasons for such differences constitute a fascinating question. It is to be hoped that a subsequent edition will give the individual equivalents developed for trucks and other classes of producers durables. We could then determine whether it is weighting that produces this result, difficulties in the procedure of using cost of passenger car per kilogram, or some other circumstance. Since the authors used prices of producers durables in deflating defense goods expenditures, the differences between these two sets of data are also of interest. (Presumably the sharp differences reflect relatively small amounts of equipment being purchased in France and Italy.)

One of the more questionable relationships between purchasing power equivalents is that between total food and the vegetable and fruit group in the United Kingdom and France. That these two countries should appear to be relatively such inefficient producers of fruits and vegetables is a striking presumption worth further study.

E. Almost inevitably the study systematically tends to understate the internal purchasing power of European currencies and the comparative GNP of the four countries. This is so despite the authors' manifest intentions and extreme care. It occurs because of a tendency to write off the greater volume of services and the higher level of quality characteristic of certain European goods.<sup>11</sup>

In summary, this major study should supersede a variety of less careful, less thoughtful, measures. It puts the onus on those who prefer to use exchange rates to deflate national incomes rather than the "equivalents" developed here with such admirable care and effort. And it provides a storehouse of data on differences which prevail between the production patterns and price relationships of these five countries. It is to be hoped that future editions will extend comparisons to still other OEEC members. It is even more to be hoped that, free of the inevitable limitations on an official study, the authors will collabo-

<sup>9</sup> In future editions it would be desirable to measure residential construction cost differentials not merely from the cost of specified brickwork, painting and plastering operations (pp. 194-95) but also to include some allowance for carpentry—the latter accounting for a much larger share of construction costs in most countries.

<sup>10</sup> The ratios shown for tobacco are those at factor cost—hence neither subsidies nor taxes should be a factor here.

<sup>11</sup> Cf. p. 88 for a thoughtful discussion of the reasons why some of this is necessary.

rate in a private study of the economic implications and meaning of the materials they have so laboriously and brilliantly put together.

STANLEY LEBERGOTT

*Washington, D.C.*

*American Foreign Assistance.* By WILLIAM ADAMS BROWN, JR. and REDVERS OPIE. (Washington: The Brookings Institution. 1953. Pp. xii, 615. \$6.00.)

From 1941 through 1952 the United States extended to foreign nations nearly \$100 billion in foreign assistance. The funds involved were granted under many labels, for a variety of purposes, and under widely divergent conditions. To recount the history of this unprecedented international flow of funds, and to assess its effectiveness, is the task undertaken by the authors of the volume under review.

Clearly, no study of such scope can satisfy the individual curiosities of all readers, but one can predict without cavil that this will be a standard work on the subject for years to come. It brings together data from a wide range of sources and presents them in intelligible and useful form; it weaves a coherent history of the events and of the motives which prompted such largesse; and it singles out a number of places where American actions may not have been optimally attuned to long-range American interests.

The story, as unfolded here, is one of the reaction, sometimes sluggish and sometimes impetuous, of United States policy to changing conditions in the world of international affairs. Wartime assistance began with a trickle, but became a flood with America's entry into the conflict. Postwar assistance was initially based on the notion that transitional relief was all that was needed, but gradually shifted to the larger-term aims embodied in the European Recovery Program. These aims, themselves conditioned by mutual security motives, were in turn modified after the Korea outbreak, when more strictly military objectives again became dominant.

The appraisal or critique of the programs given by Brown and Opie stays broadly within the framework of the history. The early wartime efforts were too niggardly; recent emphasis on military as opposed to economic assistance may be too short-sighted; during the recovery program period emphasis was placed on prospective dollar deficits rather than the prospective contribution of additional funds to the economic strength of the free world. In addition, the authors take potshots at American attempts to tie strings to grants: for example, the provisions of the Economic Cooperation Administration requiring priorities to be given to the United States as a source of supply for agricultural commodities declared "surplus" here, the provisions regarding shipment of ECA cargoes in American ships, and the arbitrary restrictions on East-West trade that have from time to time been built into the assistance machinery.

As may be evident from the above, this volume is essentially a history written by economists with good professional insight and judgment. It is not, and does not pretend to be, an economic analysis of the effects of American assistance on the war effort, the recovery of Europe, or the recent defense mobilization. Accordingly, those who want to know how many of the billions

given in foreign assistance were "wasted," and in what sense, or whether particular branches of our aid program should have been expanded at the expense of others, will have to wait for further studies in this vast and relatively unexplored field.

ARNOLD C. HARBERGER

*University of Chicago*

*Germany's Comeback in the World Market.* By LUDWIG ERHARD. (New York: Macmillan. 1954. Pp. 271. \$4.50.)

Western Germany is not a free-trade country, inasmuch as its tariff barriers remain relatively high; but it led the European nations in dismantling the prevailing system of import quotas when the European Payments Union offered the opportunity. If the author, Ludwig Erhard, German Minister of Economic Affairs and professor of economics at the University of Bonn, could have his way, currency convertibility, the abolition of import quotas, and the establishment of realistic exchange rates, as well as unconditional most-favored-nation treatment, would be objectives to be achieved by all nations as soon as possible.

Erhard's economic credo pervades the whole book, which was prepared in the foreign trade division of the Federal Ministry of Economics under Herbert Gross as director of research; however, the study is largely a chronological account of German foreign-trade policy, from its postwar beginnings in the three Western zones of occupation, through the summer of 1953, when the book was first published in the German language. Great stress is laid on the importance of trade and clearing agreements in Germany's expanding trade, and considerable space is devoted to the part played by such agreements in helping to solve problems relating to specific countries and currency areas, and in creating new opportunities for German trade. The book should therefore be very useful for anyone dealing with German foreign-trade questions, since the background information and discussion of procedure are extremely comprehensive and are accurately done. However, while focusing on the institutional framework of Germany's trade relations as it evolved after the war, the book only in passing treats certain basic factors such as Germany's monetary reform and its monetary and fiscal policies, which appear to have contributed even more to the country's comeback in the world economy than has the institutional framework.

Interesting from a historical point of view are the sections on foreign trade policy under the Allied Military Government, a period now almost forgotten. The study is quite critical of Allied Military "bureaucracy," holding that it tended to hamper German advances in foreign trade during that period. It acknowledges, however, that prior to the establishing of a sound currency by the currency reform of 1948 (which Erhard helped to prepare), not even "superpatriots" could have achieved a larger volume of trade; it also gives credit to the United States as being the only one of the three Allied powers that urged from the very beginning a return of foreign trade to private hands.

As for the future of German foreign trade, Erhard sees Germany's great opportunity in the expanding markets created by the desire of the less-developed nations for greater industrialization. Germany's technological genius

and its ability to produce the desired equipment should promise a great future for German capital exports to such nations.

H. J. DERNBURG

New York, N.Y.

### Business Administration

*L'Organisation rationnelle de la distribution (moyen de stabilisation économique)*. By ERIC BOVET. (Neuchâtel, Switzerland: Delachaux & Niestlé. 1954. Pp. 268.)

The author has two purposes: (1) an explanation of the ways in which current methods of distribution in industrialized countries operating under a "free enterprise" system create substantial economic instability and contribute to economic stagnation, and (2) a prescription of policies which will beneficially change methods of distribution.

Major evils of systems of distribution such as exist in the United States and France are, according to Bovet, a failure of changes in demand by ultimate consumers to be communicated promptly and accurately to manufacturers through the channels of distribution. This deficiency in our pricing mechanism creates substantial fluctuations in inventories at all levels in the channels of distribution with cumulative effects that tend to generate self-reinforcing upward or downward movements in the economy. The characteristics of our distributive system which create the malfunctioning of the pricing mechanism are the lack of a pervasive and relatively complete vertical integration and the practice of producing in anticipation of orders rather than to order. These characteristics of our distributive system have become increasingly important and detrimental as the technology and scale of manufacture have changed. In Bovet's view, distribution has not adapted itself to current methods of manufacturing.

There will probably be little disagreement with the view that inventory fluctuations have contributed significantly in recent times to economic instability, or even that instability and the uncertainty which it creates can be deterrents to economic growth. Nor will Bovet fail to find substantial support for his view that legal barriers to vertical integration such as have been created in this country by decisions such as the one handed down in the A & P case are not in the public interest.

It is much more difficult to see merit in the author's diagnosis of the ills of our economic system and especially in his prescriptions for their remedy. Bovet's major themes are that we should integrate vertically and produce to order rather than in anticipation. He admits, however, that vertical integration is often impracticable, as is also production to order. He provides no guides as to how far we should go in these directions, and thus we have no new principles by which entrepreneurs can order their behavior. There has never been much promise in any prescription which seemed to imply that businessmen should seek anything other than what they regard as the well-being of their enterprises.

Bovet does make one specific recommendation of a secondary nature which seems wrong rather than merely not helpful. He recommends that vertically

integrated firms price their products to ultimate consumers on the basis of incremental production costs at the first stage in the process of production rather than on the basis of the cost at the last stage as defined by internal transfer prices. If transfer prices are determined by free negotiation between the integrated units and in the presence of real alternative markets for both buyer and seller, the transfer price thus arrived at is a better (more profitable) guide to managerial pricing than is the current incremental production cost at some earlier stage in production. The use of transfer prices, so determined, seems to be increasing in American business. Even when the conditions just described do not exist, there seems no reason to have regard exclusively for incremental costs at the earliest stage in production as opposed to considering all costs of producing the ultimate market values that are demanded by consumers.

JAMES H. LORIE

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### Industrial Organization; Public Regulation of Business

*The Attack on Big Business.* By J. D. GLOVER (Boston: Division of Research, Graduate School of Business Administration, Harvard University. 1954. Pp. xvi, 375. \$4.00.)

*Big Enterprise in a Competitive System.* By A. D. H. KAPLAN (Washington: The Brookings Institution. 1954. Pp. xii, 269. \$4.00.)

A renascent interest in big business has arisen from quarters holding disparate social points of view. Hence, it is not surprising that while Glover was in the process of preparing his thesis that nearly everybody is *against* big business, several books and a spate of articles appeared in its defense.<sup>1</sup> Unfortunately in a way for Professor Glover one such book, Mr. Kaplan's, is reviewed here with his.

Even the most ardent critic of bigness would probably concede during his moments of dispassionate reflection that the composite attack on big business can scarcely claim the support of impeccably coherent logic: big profits suggest monopoly, small profits inefficiency; high prices look like consumer plundering, low prices a tactic for driving out competition; expansion heads off new competition, failure to expand is output restriction. Some scholars attack big business as *monopolistic* but support big labor, big government, and cartelized agriculture; and some condemn big business price rigidities as non-competitive but champion Fair Trade. Glover's book is essentially a catalog of all these attacks and many others.

But we may concede to Glover that big business has been attacked with irresponsibility and still not erase the problem of bigness. To a society that leaves the determination of the composition and level of a substantial share of its national product to the competitive pricing system, bigness in a market sense is a problem, and one that does not diminish—much less disappear—because critics of bigness have sometimes chicaned. Some of the attacks Glover

<sup>1</sup> For example, see D. Lilienthal, *Big Business, a New Era*, and J. K. Galbraith, "The Defense of Business: A Strategic Appraisal," *Harvard Bus. Rev.*, Jan.-Feb., 1954, XXXII, 37-43.

distains are valid, and some others are invalid not because of confusion of the attacker, but because they are not attacks on bigness. Glover fails to distinguish between attacks on bigness and attacks on a host of other things ranging from individualism (p. 232) to industrial society (p. 201) and urban life.

For this reason his final chapter of prescriptions for big business action holds little promise of quieting the criticism. No business action can simultaneously quiet criticisms from such a variety of sources as the Marxists, the apostles of atomization, the critics of business, the critics of individualism, Wayne Morse, T. S. Elliot, John P. Marquand, and George Bernard Shaw.

In contrast to these attackers, Mr. Kaplan has looked upon the structural aspects and performance of big business and found neither a cause for alarm. Big business has, on balance, simply kept pace with the rest of the economy: the largest 5 per cent of the manufacturing establishments hired 55.3 per cent of all manufacturing employees in 1914 and 62.3 per cent in 1947 (the slight increase Kaplan attributes to the second world war); the 100 largest industrial corporations held 24.6 per cent of all industrial assets in 1909 and 26.7 per cent in 1948; they earned 31.1 per cent of all industrial corporate profits before taxes in 1909 and 30.1 per cent in 1948; and between 1929 and 1948 their percentage of all industrial corporate profits after taxes declined from 44.03 per cent to 29.85 per cent. The number of firms is growing more rapidly than total population; noncorporate enterprises still account for almost as large a percentage of the national income as in 1929; and over 70 per cent of the gainfully employed are found in the smaller business units or in the non-business field. In short, the giants have not taken over the country and there is no evidence that they are doing so.

Moreover, big business has performed well and exists in a constant state of competitive turmoil. Kaplan's principal criticism of big business on this score is that it has not matched its brilliance of innovational change with "commensurate ingenuity and boldness in sloughing off parts that no longer gain in efficiency by being integrated with the giant enterprise" (p. 227). But Kaplan concludes: "Big business has not merely been kept effectively subject to a competitive system; on the whole it has also made an essential contribution to its scope, vitality, and effectiveness" (p. 248).

On many of these subjective conclusions Kaplan may be right. However, to this reviewer, his conclusion on the fluidity in the upper echelons of big business rests on unconvincing evidence. Kaplan infers dynamic competition from the high turnover among the largest 100 industrial firms. He divides the 100 largest firms into 21 broad industry classifications, and notes that of the 100 comprising the 1909 list only 36 appear on the 1948 list, and that within each of the 21 industries considerable interchanges and turnovers in positions have occurred. Except by footnote references, he fails to point out that 6 of the original 100 disappeared via the merger route, and that 6 of the largest 100 on the 1948 list resulted from the dissolution of American Tobacco and Standard Oil. However, we may excuse these omissions from his computations on the grounds that the results are neither materially affected nor indicative of competition anyway. It should come as a surprise to no one that between

1909 and 1948 steamship, express, sugar refining, locomotive, and ice companies gave way to producers of automobiles, aircraft, motion pictures, electrical appliances, and rubber tires.

Because Kaplan has used such broad industry classifications we would expect, for the same reason, to find considerable firm turnover and disparate rates of growth during this period within each of the 21 industries. For example, transportation equipment includes producers of farm machinery, pullman cars, locomotives and parts, steamships, automobiles, and aircraft. Within this industry group in the course of 40 years American Locomotive, Baldwin Locomotive, Pressed Steel Car, Railway Steel Spring and American Shipbuilding disappeared from the 100 largest and were replaced by General Motors, Ford, Chrysler, and Curtis-Wright. The fluidity at the top is clearly a result of too broad industry classifications.

The reviewer, with no intention of turning a book review into a major research project, rearranged the firm ranks in accordance with somewhat narrower industry definitions and constructed the following table:

<i>Firm and Industry Classification</i>	<i>Rank</i>	
	<i>1909</i>	<i>1948</i>
<i>Iron and Steel</i>		
U. S. Steel	1	1
Colorado Fuel and Iron	2	—
Lackawanna Steel	3	— <sup>(a)</sup>
Republic	4	3
Bethlehem	5	2
<i>Copper</i>		
Amalgamated (Anaconda)	1	1
American Smelting and Refining	2	3
Kennecott	—	2
<i>Nickel</i>		
International	1	1
<i>Meat Packing</i>		
Armour	1	2
Swift	2	1
Cudahy	3	3
<i>Dairy Products</i>		
Borden	1	2
National Dairy	—	1
<i>Transportation Equipment/Excluding Autos and Aircraft</i>		
International Harvester	1	1
Pullman	2	2
American Car and Foundry	3	3
<i>Petroleum</i>		
Standard Oil (Standard of N.J.)	1 <sup>(b)</sup>	1
Standard of Indiana	— <sup>(b)</sup>	2
Socony-Vacuum	— <sup>(b)</sup>	3
Standard of California	— <sup>(b)</sup>	4
<i>Chemicals</i>		
Du Pont	1	1

<i>Firm and Industry Classification</i>	<i>Rank</i>	
	1909	1948
<i>Coal Mining</i>		
Pittsburgh Coal	1	1
Consolidated Coal	2	— <sup>(e)</sup>
<i>Tobacco</i>		
American Tobacco	1	1
R. J. Reynolds	— <sup>(d)</sup>	2
Liggett & Myers	— <sup>(d)</sup>	3
<i>Electrical Equipment and Machinery</i>		
General Electric	1	1
Westinghouse	2	3
Western Electric	3	2
<i>Rubber</i>		
U. S. Rubber	1	2
Goodyear	—	1
<i>Lumber and Paper</i>		
International Paper	1	1
<i>Containers</i>		
American Can	1	1
<i>Retail Distribution</i>		
Sears-Roebuck	1	1
<i>Glass</i>		
Pittsburgh Plate	1	1
<i>Miscellaneous</i>		
United Fruit	1	1

<sup>(a)</sup> Merged with Bethlehem in 1922

<sup>(b)</sup> Created by dissolution of Standard Oil in 1911

<sup>(c)</sup> Merged with Pittsburgh Coal in 1945

<sup>(d)</sup> Created by dissolution of American Tobacco in 1911.

These rankings scarcely support Kaplan's conclusion that firms are constantly in danger of losing their foothold in their respective industries through the forces of dynamic competition. With but few exceptions the leaders of particular industries in 1909 and 1948 were the same firms. The interchanges in ranks in iron and steel are due almost entirely to merger, and the newcomers in petroleum and tobacco we owe to the Department of Justice. Neither large-scale merger nor dissolution is an integral part of dynamic autonomous change.

In spite of the dubious support his own data give to his principal thesis, however, Kaplan's book contains much useful information and says a great deal that makes sense. And if its readers should conclude that Kaplan has been less critical of big business than he should, they may quickly turn to Glover for a comprehensive footnote on how vigorously and viciously big business can be attacked. The reader's emotions toward big business may possibly undergo violent cyclical swings as he moves between the two books, but they will very probably come to rest in the approximate neighborhood of where they started.

JESSE W. MARKHAM

Princeton University

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JESSE W. MARKHAM

*Princeton University*

*Fair Competition: The Law and Economics of Antitrust Policy.* By JOEL B. DIRLAM and ALFRED E. KAHN. (Ithaca: Cornell University Press. Pp. xi, 307. \$4.50.)

In recent years the antitrust laws have come in for increasing criticism. Some critics have charged that the laws now restrict businessmen so much that they discourage the vigorous competition they were designed to protect. Others have asserted that the sort of competition the law attempts to establish is either unobtainable or, in view of the fine performance of an imperfectly competitive American economy, actually undesirable.

In this book Professor Dirlam and Kahn attempt to defend antitrust law against its critics. After summarizing court decisions of the last fifteen years, they conclude that the Sherman and Clayton Acts have not deviated from their historic and proper path. They admit there have been some changes—chiefly with respect to price discrimination—but they are not as great as critics have claimed.

To Dirlam and Kahn the law has not only remained essentially unchanged, but is appropriate and effective as well. Indeed, they say, the law furnishes better standards for judging business forms and practices than does economics. Much of this alleged superiority rests upon the use of "intent" to judge legality. In their judgment, legal criteria are superior both because morality may demand that some actions be proscribed irrespective of their economic effects, and because economists have not supplied useful alternative standards. As is sometimes true of fervent declarations of faith, however, confidence in "intent" is not nearly complete. For, as they put it, intent must be accompanied "by the power (actual or imminent) to restrain or exclude . . ." (p. 54). They classify court decisions according to the types of practices with which they deal: vertical integration, monopolizing, exclusive arrangements, tied-in sales, and price discrimination. I shall confine my comments to the discussions of vertical integration and price discrimination.

With respect to vertical integration, the authors make two key claims, neither of which they systematically prove or explain. First, among other things, vertical integration enables a firm with some monopoly power at one level to *increase* its monopoly power at another. Second, integration forces monopoly misallocations from one level into the others in some way that vertical disintegration would remedy. Both of these assertions are incorrect, as the literature amply demonstrates.<sup>1</sup> In support of the first proposition the authors offer the slender defense that businessmen may not actually try to maximize profits. With an effective economic hypothesis thus abandoned without replacement, it is not surprising that analysis and policy recommendations suffer.

Their economics is unorthodox and mysterious in still other ways. At one point, for example, they are led into what seems to be an attack on the ra-

<sup>1</sup> See, for example, J. J. Spengler, "Vertical Integration and Antitrust Policy," *Jour. Pol. Econ.*, Aug. 1950, LVIII, 347; M. A. Adelman, "Integration and Antitrust Policy," *Harvard Law Rev.*, Nov. 1949, LXIII, 27; R. Bork, "Vertical Integration and the Sherman Act: The Legal History of an Economic Misconception," *Univ. Chicago Law Rev.*, Autumn 1954, XXII, 157; "Comment: Vertical Forestalling under the Antitrust Laws," *Univ. Chicago Law Rev.*, Spring 1952, XIX, 583.

tionale of market pricing. In their words, "Government intervention outside the antitrust laws is probably the only way of assuring fairness in the distribution of scarce materials" (p. 159). If they are correct, it is a discovery worth explaining in detail. Unfortunately, however, they develop neither the details of this deficiency of a pricing system nor the working of their remedy for it.

The discussion of price discrimination suffers in much the same way. In addition to reviewing the standard criticisms of the practice, the authors make the dubious argument that discrimination is bad because it may involve "recoupment." This argument asserts that when a seller discriminates in price, the "lower price to one buyer ordinarily means the price to other buyers will be raised correspondingly" (p. 208). This notion rests upon the explicit assumption that sellers do not seek to maximize profits. Since that hypothesis is consistent with virtually any kind of behavior, it is often unclear why the authors approve or disapprove of a particular court decision or business practice.

After analyzing the cases, Dirlam and Kahn conclude that the antitrust laws have not seriously interfered with efficiency. Not only have the antitrust laws not gone too far; in the case of concentrated industries, "the solution . . . would be to break up excessively concentrated market structures" (p. 281). It is anomalous that the authors find themselves capable of making the same sorts of economic judgments that they find economists generally incompetent to make.

They do find the Robinson-Patman Act to be too confining, and, in a general way, advocate loosening its restrictions somewhat. They apparently advocate this on economic grounds, but at the same time warn that "considerations of equity cannot be abolished in the name of effective competition" (p. 277). Just where this leaves the matter is not absolutely clear.

This book thus has serious defects. Although they profess little confidence in economics, the authors deal with problems that require it, and indeed sometimes advocate its use. Unfortunately the economic analysis they do adopt is weak; there are errors here that betray an inadequate understanding of the economic tools the authors condemn. One example is their contention that the theory of pure competition is inapplicable even when numbers are "infinite" (p. 233).

In addition to technical errors and irrelevancies, there is much here to confound anyone interested in scientific methodology. Terms like "dominance," "strategic superiority," and "leverage," though used often, are not explained. Furthermore, the authors sometimes end up evaluating different variables from those whose analysis they promise.

Although this book recognizes a fascinating and important problem, its approach falls between chairs. It might systematically have defended the law on purely moral, ethical, or other noneconomic grounds. It might, alternatively, have analyzed the law purely in terms of economic principles, or by the systematic use of both economic and noneconomic standards. One major defect of this volume is that it mixes, in unspecified proportions, vague moral values and unsystematic economics.

The book is not without merit. Probably most useful are the summaries

of recent cases and discussions of the ideas of many writers in the field. The substantial bibliography and table of cases are also helpful. More important, we may predict that this volume will add new interest to the stimulating controversy of which it has now become a part.

JOHN S. MCGEE

*The University of Chicago*

*Die Neuordnung der Eisen- u. Stahlindustrie im Gebiet der Bundesrepublik Deutschland ein Bericht der Stahltreuhandervereinigung.* (Münich and Berlin: C. H. Beck. 1954. Pp. xxii, 872. DM. 48.)

This report of the German subgroup operating as the executive arm of the allied control of the German steel industry, between 1949 and 1952, gives an account of its stewardship, which ended with the coming into operation of the Schumann Plan. Together with a description of the (coal and) steel industry, it gives a detailed account of the motives, problems, development, and results of the Allied policy concerning the West German steel industry. Among other things, very interesting insights into a pattern of "workable" imperfect competition emerge. Currently, the technological dynamics in the steel-using industries seem largely to influence the rational pattern for structure and business procedure in the steel-making industry.

The reporting group, with the approval of the allied authorities, did not function merely as a passive subordinate organ of Allied policy but took an active part in the planning and acted as the guardian of justifiable German interest.

The views and actions of the Allied authorities, of the German combines and the trade unions as well as the views of corporation lawyers and experts in international law and of German economic and technical experts are presented if not in the text, then in the annexes which cover about half of the volume. Allied ordinances and decisions concerning the reorganization and the exchanges about this between the industry, the controlling authorities, the German government, the trade unions, etc., are reproduced. A detailed account of the structure of the combines before and after the reorganization is given as well as a description of the pattern of the so-called codetermination between management and labor.

In assessing the merits and demerits of the organization and policy of the industry itself and of the success or otherwise of the Allied reform effort, it must be remembered that the industry had to meet frequent radical changes in environment, and that the goal of Allied policy has shifted from the main emphasis on disarmament and dismantling to one of deconcentration and decartelization and finally to one of reconstruction of an efficient, highly competitive industry and ultimately even to partial restoration of armament production capacities. Finally, the industry, together with the closely united German coal-mining industry, entered into the Western European coal and steel union in the fold of the European Payments Union and the Organization for European Economic Cooperation. This marks the beginning of the end of allied control.

A full appraisal of the industry's performance and of the Allied effort to

improve on it is impossible in the space here available. This reviewer was left with the impression that while the breakup of overly large combines, especially in the case of the German United Steel Works with its 177 subgroups, was called for in the interest of more initiative and flexibility of management, the originally planned horizontal breakup frequently overshot the mark of what was sound in terms of efficient production and marketing. The full separation of the steel works from the vertically integrated coal mines and from the affiliated steel-processing combines (shipyards, construction works, wire and tube industries, etc.) if carried out as planned, would have vastly reduced the competitive capacity of the industry by comparison with its horizontally as well as vertically highly integrated European and American rivals. Also there were inconsistencies in objectives. In view of the rapidly changing general conditions under which the industry operates, the planned schemes for plant structure seem too final and too much oriented to the conditions of a closed German economy, while on the other hand the final financial reorganization is left to the decision of a future independent German government. The only partially accomplished breaking up of vertical and horizontal combines in some instances left truncated bodies which in order to regain efficiency, would have to be reintegrated with others, which is against the purpose of Allied policy, or would have to be supplemented by new installations, which would mean misinvestment of very scarce capital.

In fact, with the end of effective Allied control a movement of reconcentration has already set in. The prewar formal cartels, however, have not made their reappearance.

HERBERT VON BECKERATH

*Duke University*

*Competition and its Regulation.* By A. G. PAPANDREOU and J. T. WHEELER. (New York: Prentice-Hall, 1954. Pp. vii, 504. \$6.50.)

Professors Papandreou and Wheeler have innovated with considerable success in their new textbook, designed primarily for undergraduate courses in public control of business. Their innovations consist of the presentation of certain conceptual developments of recent years heretofore confined to more advanced and specialized works or to the journals, and a somewhat novel organizational structure in presenting the material. The volume is divided into two parts, the first presenting the analytical tools deemed useful by the authors for understanding the regulation of competition; the second presenting a picture of the development of antitrust policy primarily as evidenced in the important decisions of the courts.

In the theoretical portion of their book Papandreou and Wheeler begin with the concept of the market group, defined in terms of cross-demand and cross-sale schedules. (The cross-sales schedule is that portion of the cross-demand schedule which includes those prices that the rival can make "effective" in the market, *i.e.*, where he has sufficient capacity to make a lower price stick by supplying the increased quantity demanded.) The market group concept and the industry concept (defined on the usual basis of technology and product similarity) are combined to produce the basis for delineating the various

market/industry subgroups which are distinguished in the subsequent analysis. This is followed by an analysis of the crucial decisions of the firm including those as to price, output, product mix, selling effort, investment and price discrimination. The authors rely exclusively on the "break-even chart," and make no use of the traditional marginal analysis.

The innovation which will probably attract most attention for this volume is its presentation of firm behavior in "circular" (oligopolistic) markets in the framework of the theory of games. Lest some potential users of this volume be frightened by this, it should be emphasized that only the simplest and most rudimentary skeleton of game theory appears and no mathematical material is even mentioned. The authors' use of game theory is nothing more than a convenient and helpful way of looking at the various forms of behavior which may appear in oligopolistic markets.

The chapters in the first part, which in addition to the subjects already mentioned discuss buyer-seller relations, entry, and selling outlay and product type in circular subgroups, are designed not only to develop concepts which can be used in analyzing particular cases, but also to serve as the basis for consideration of "Criteria for Public Policy." This serves as the bridge between the two parts of the book and indicates the standards by which anti-trust policy is to be evaluated in the second part. The criteria presented are the authors' own variant of "workable competition." With others who identify their views by this term they are unable to come up with a very precise set of rules and they are at pains to confess the unsatisfactory status of their criteria.

Papandreou and Wheeler argue that the public authority should aim at preservation of freedom of competition or "potential competitive pressure" and that to do so the attempt must be made to assure that (1) entrance is free, (2) no firm is coerced to withdraw from the market, and (3) firms do not fix the outcome of the "game" through cooperative action. They then rate the major types and subtypes of market group behavior (independence pattern, cooperative pattern, restrictive pattern, interference pattern) which their analysis has delineated. They recognize the extreme difficulties which such a general prescription faces in applying it to such problems as firm size, vertical integration, dissolution, etc.

The second part of the book is organized along familiar lines under such headings as horizontal coalition, aggressive behavior, firm size, monopoly, fusion (merger), unfair competition, various types of price discrimination, and resale price maintenance. Under each of these headings the historical development of the law is presented with frequent reference to the important cases and extensive quotation from the opinions of the courts. The volume ends with a chapter on enforcement.

It is impossible in the scope of a review to mention all of the points at which the reviewer would like to give a special word of commendation or voice a word of objection or disagreement. In the first category, I would like to compliment the authors' selection of cases and quotations from cases. They have resisted the temptation to reproduce a legal case book and have kept the size of this part of the work surprisingly small, yet they have been able to cover most of the points significant for the economist in evaluating trade regu-

lation. This reviewer has found also that the game concepts are pedagogically useful in helping students understand the nature of what we call "the oligopoly problem." The classification of market and behavior types is also on the whole very satisfactory. Almost everyone teaching in this field has his own particular viewpoint. This book is so organized, and in particular the cases so well selected, that there is ample opportunity for interpretation and evaluation different from the authors'.

On the other side of the ledger, I would question the wisdom of eliminating marginal theory completely, since the break-even analysis proves particularly cumbersome when dealing with some problems such as price discrimination. One's taste here will depend on the training of the students. If most of the students have only a principles course behind them, and particularly where the principles course devotes little time to price theory, then Papandreou and Wheeler's approach may be an asset.

Since the game theory concepts are useful, it seems unfortunate that the authors did not push them just a bit further. One of the most useful parts of game theory for oligopoly analysis appears to be its suggestions as to the rules for coalition formation. While it is probably necessary to introduce the concept of the mixed strategy to deal adequately with this topic, the authors have demonstrated sufficient expositional skill to make one wish they had been more ambitious. Perhaps they will at least include an appendix on this subject in later editions.

A final word regarding the use of this book: it is short enough so that in a semester course other materials can be assigned and discussed. This reviewer has found a desirable complement in one of the industry study books. Papandreou and Wheeler present no empirical material illustrating their analysis except for the snatches of descriptive material included in some of the court opinions quoted. Another possible supplement to this book, and one some teachers will find desirable, is one or more sources which discuss the noncompetitive sectors of the economy where regulatory commissions are relied on instead of the competitive market. The absence of any discussion of other than the competitive sector of our economy will be regarded by some as an unfortunate omission.

In textbooks the proof is in the teaching: I have used Papandreou and Wheeler once and I would use it again. In addition to its usefulness as a textbook it provides a clear and relatively accessible expression of the viewpoint shared by many economists towards the problems discussed and as such may appeal to nonspecialist economists and interested laymen and lawyers.

WILLIAM M. CAPRON

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**Land Economics; Agricultural Economics; Economic Geography**  
*Agricultural Policy: Farm Programs and National Welfare.* By RAINER SCHICKELE. (New York: McGraw Hill. 1954. Pp. x, 453. \$6.50.)

This book is a very comprehensive presentation of the U.S. farm problem. In contrast to other discussions of this topic in which goals of economic policy

are not formulated with full clarity, Schickele offers a thorough discussion of these goals. He postulates "maximum social product" and "optimum income distribution" as basic norms. A number of readers may not object to the norms *per se*, but to the rather involved way in which they are developed.

Government programs are grouped into, (1) programs for improving resource allocation, (2) farm price policy, and (3) programs for improving income distribution. In the light of the goals of agricultural policy as set up by the author all three groups deserve the painstaking consideration given to their evaluation: the greatest interest, though, centers around the author's analysis of farm price policies.

Among the programs for improving resource allocation the author discusses soil conservation in great detail and shows that a number of conservation measures did not achieve the desired result of improving conservation practices. Conservation payments were frequently made for practices on land which did not require special conservation techniques. The author points out that the public is entitled to clear-cut accounting of costs and accomplishments of various practices; but at the same time he is aware of the administrative difficulties of controls. It would appear that the educational work and the stimulation to individual action are probably the most effective part of the U.S. conservation program.

The author's discussion of farm price policies will arouse objections from all sides. Opponents of support prices for special commodities have shown convincingly that these measures neither promote better income distribution, nor improve resource allocation. Schickele admits that support prices of special commodities have their shortcomings, but he explains their dominant role in all farm programs by their immediate effect on farm incomes: "Farmers look at farm prices much the way industrial workers look at wages." Schickele recommends support prices for wheat, cotton, corn and citrus fruit, pointing out that these commodities are subject to particularly great price fluctuations and play a decisive role in the economic life of wide areas. In addition to price supports for the above mentioned crops, Schickele favors forward prices and production goals for a large number of farm products for which production adjustments can be achieved by shifting variable factors.

The author is well aware of the unsatisfactory effect of the existing support system on income distribution. He therefore suggests a number of measures to alleviate poverty on submarginal farms, and to improve economic prospects of middle-sized farms. He is especially concerned with improving credit facilities, with crop insurance, and with variable debt payment. He analyzes programs to safeguard farmers' ownership of land, and encouragement of farm cooperatives.

While the author's recommendations for raising income standards in general will be accepted by most students of the U.S. farm program, it seems obvious that Schickele is inclined to underrate the effect of the support-price system on the U.S. economy. High support prices are responsible for the accumulation of enormous surpluses in the hands of the government (about \$4.2 billion of surplus goods owned by the Commodity Credit Corporation plus \$2.9 billion of outstanding loans at the end of 1954). Wheat prices are supported at levels

which cover costs of production on submarginal land and far above costs of low-cost producers in the fertile wheat areas of our country. The high level of these prices has encouraged increased production of wheat all over the world. The disposal of U.S. surpluses on domestic and foreign markets poses one of the most perplexing problems of our economic policy.

MARTHA STEFFY BROWNE

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### Labor

*The Theory of Collective Bargaining: A History, Analysis and Criticism of the Principal Theories Which Have Sought to Explain the Effects of Trade Unions and Employers' Associations Upon the Distribution of the Product of Industry.* By W. H. HUTT. (Glencoe, Illinois: The Free Press. 1954. Pp. 150. \$3.00.)

At a time when the issues of imperfect markets and countervailing power are being widely debated, the reprinting of this book, originally published in 1930, will be of interest to economists. For despite a wealth of recent institutional interpretations of unionism, the *economic* claims of collective bargaining have not received much theoretical attention since the period of active discussion that extends from Thornton in the 'sixties to the Webbs in the late 'nineties of the nineteenth century. So far as the economics of the issues are concerned, we have had a mélange of partial cases and attacks on marginalism, whose collective burden has been to show that unions can raise wages without decreasing employment, but which have not explored the implications for the whole economy. One might even say that of late labor economists have become so preoccupied with the terms of trade under collective bargaining that they have forgotten the whole question of the gains of trade from specialization and division of labor.

Professor Hutt has tried to remedy this situation. He holds that the economic case for collective bargaining reduces to two contentions: (1) if no collective bargaining, the workers' market disadvantage would enable the owners of other productive agencies to appropriate income that would otherwise go to labor; and (2) with collective bargaining workers *in general* can appropriate income from the owners of other agents. Hutt rejects both contentions.

The idea of labor's market disadvantage, he argues, stems from Adam Smith, who tied it to his subsistence theory. First, Smith held that masters combined to force wages down, for which there is little evidence. Second, Smith said that masters also combined to keep wages low, which Hutt says merely means that employers then believed in a "natural rate" and were reluctant to raise wages, though Smith himself made the exception that wages would rise under competitive pressure whenever the rate of saving (reserves) outran the rate of population growth.

Third, Smith claimed that the masters' "necessities" were less pressing than those of the workers. Somewhat contradictorily relative to his exception just noted above, Smith based this notion on the difference in relative reserves.

Thornton and others amplified and further confused this idea, referring vaguely to "inequality of bargaining power," inability to monopolize the sale of labor power, or the perishability of labor services. Yet perishability is equally true of much capital (Nicholson), while storability of labor power might at times actually prove a handicap (N. G. Pierson).

In the 'sixties there also emerged the idea of the indeterminateness of the wage bargain: Thornton and Mill suggested that if two parties bargain in isolation, there may be no unique market-clearing price. Edgeworth developed this idea with his superimposed indifference maps of 1881, but later discounted its practical importance.

Jevons took a somewhat different view of indeterminacy, basing it upon the exchange of a unique and indivisible good sold in perpetuity by a monopolist. Although he did not relate the case to the labor market in his work of 1871, in his *State in Relation to Labour* (1882) he treated combination bargaining as involving a "single object" (indivisible unit), and declared that supply and demand analysis was not applicable. As Hutt says, however, labor quantity would still be a variable, even if the wage were indeterminate. This objection stands unless one presumes Fellner's "all-or-nothing" case, where the wage and quantity of employment are jointly bargained for.

Hutt admits that particular unions can raise wages—for small segments of the wage-earning population but not for *all* labor. One method is to exclude competing units of labor and indirectly some of the cooperants, and if possible to prevent the excluded factors together from re-entering the trade. Another involves the rare case of inelastic labor demand, where consumers and owners of immobile cooperant factors are both exploited. However, the immobility of capital is not permanent: its long-run supply is highly elastic because new capital will avoid a badly squeezed trade and old investments will be liquidated. Further, whatever the method of forcing up wages, the decreased scale of the industry will in time also be accompanied by substitutions of capital for labor.

Hutt also denies that labor in general is immobile and so could be exploited by capital as a whole. Monopsony is at most a specialized and temporary situation and will be overcome by entry of new firms seeking the added profits of temporarily depressed wages.

The argument yields two general conclusions. One is that workers as a whole do not suffer any inherent bargaining disadvantage that, in the absence of collective bargaining, would force wages below competitive levels and presumably towards "subsistence," transferring income to owners of other factors. The other is that collective bargaining cannot extract income from these other factors, converting it to wages for the benefit of labor as a whole. Partial wage gains are not taken ultimately from profits or interest but from consumers, who will pay higher prices for eventually decreased outputs in the affected trades. Since the consumers who lose are mainly wage-earners themselves, what collective bargaining at most can do is to redistribute income within the working class itself. So Hutt concludes that the economic claims of collective bargaining do not stand up. Its justification lies in other directions: regulating hours, conditions, and personnel actions—a domain where the discipline of the market is ineffective.

One possible criticism of the argument is that it probably overrates the mobility of labor. Recent empirical studies would so suggest, though they tend to overstress the range and severity of monopsony.

There is also a question of fact: whether unions in general in this country have had the power and have actually exerted the drastic cost pressure usually attributed to them. This reviewer suspects the over-all answer is negative, that unions pay more attention to demand conditions than is usually credited to them. The whole question has been obscured by too much emphasis upon supposedly inflexible "patterns" generated by and limited to periods of inflation (1941-48, 1950-53) and by the special case of the United Mine Workers.

The author's framework is Marshallian and passes over complex questions of wages and employment during major business fluctuations. The gap is serious, though extenuated because the book was written in 1930. Moreover, while cyclical swings pose new questions outside the neoclassical framework, they do not destroy the useful predictions yielded by the analysis for the important problems with which it is properly concerned. If anything, Hutt might have extended his treatment to consider the connections between capital formation, innovation, population growth, and real wages, matters that tend to be overlooked in our current preoccupation with short-run cases.

While there are many important aspects of unionism that lie outside the domain of economic analysis, this book is a useful and needed reminder that collective bargaining carries economic implications after all. Thus its appearance is most timely.

GEORGE H. HILDEBRAND

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*Labor Mobility and Economic Opportunity.* (New York: John Wiley and Sons, 1954. Pp. viii, 118. \$3.50.)

There has been some tendency in the past to regard the economics of labor as coincident with the economics of wages. It is gradually becoming clear, however, that the subject includes a second major area—the dynamics of the labor force, the processes of labor mobility, and the structure of labor markets. Pioneer research on these matters was initiated at several American universities during the nineteen thirties. Since 1945, it has flourished on an expanded scale at Pennsylvania, Massachusetts Institute of Technology, Yale, Cornell, Minnesota, California, and a number of other centers. The movement has spread abroad, and one hears of labor mobility studies in London, Stockholm, Rotterdam, Marseilles.

The present volume, sponsored by the Labor Market Research Committee of the Social Science Research Council, marks a stage of consolidation in this general movement. It consists of a series of related essays by E. Wight Bakke, Philip M. Hauser, Clark Kerr, Charles A. Myers, Gladys L. Palmer, and Dale Yoder. Taken together, these essays provide a good review of accomplishments to date and of unresolved issues in the rapidly expanding area of labor market research.

It is not possible in a brief review to give each essay the attention it really deserves. Philip Hauser's chapter on "Mobility in Labor Force Participation"

provides the best summary available anywhere of what we know and do not know about secular, cyclical, and other types of shift in the United States labor force. It includes also a review of wartime changes, and some discussion of labor force projections from the present to 1975. The chapter is rich in statistical information and in footnote references to other sources. The author's appraisal of previous work is penetrating, fair, and candid in pointing out areas of ignorance and unresolved issues.

Clark Kerr, in a brilliantly written essay on "The Balkanization of Labor Markets," indicates some of the ways in which the "natural" or "free" labor market is being altered by the development of institutional rules, particularly rules arising from trade unionism and collective bargaining. He develops the interesting thesis that craft and industrial unionism lead to quite different patterns of labor mobility—in the craft case, horizontal movement at the same occupational level among a variety of employers, and in the industrial case strong attachment to a single employer but with the possibility of vertical movement to better jobs within the firm. These two patterns he terms respectively the "guild" and "manorial" principles of labor market organization. This is only one of several stimulating hypotheses which Kerr propounds as a spur to future research activity.

The core of the book, in a sense, consists of three essays by Charles Myers, Gladys Palmer, and Dale Yoder, which report findings from the authors' studies of mobility within local labor markets. Any student who wishes a bird's-eye view of the present state of knowledge on this subject could not do better than to begin with these chapters. The scholars who have been working on labor mobility in various parts of the country, among whom this reviewer is happy to be included, have made an unusual effort to keep in close touch as their work has proceeded and to achieve some comparability of hypotheses and research techniques. The SSRC Committee, the annual conferences at the University of Minnesota, and a number of other institutional devices have contributed to this end. In consequence, recent research on labor mobility has been "additive" to a degree which is rather unusual in social science. As one examines these three chapters, one finds an encouraging number of conclusions which have been confirmed in several studies carried out at different times and places.

One also finds some interesting conflicts in the evidence, or at any rate in the interpretation of the evidence by different scholars. Thus Myers writes: "... both studies point to the fact, also found by Reynolds and Shister, that there is very little systematic search for jobs by workers, or weighing of job alternatives" (p. 76). Miss Palmer, on the other hand, comments: "One gains the impression from a review of the Philadelphia work history records that the workers concerned had a considerable knowledge of labor market conditions and pursued their occupational careers in a purposeful fashion. . . ." (p. 62). This paradox, as both authors realize, may turn out to be more apparent than real. The groups of workers studied were rather different in the various cities, and the Palmer studies emphasized work experience over a decade or more rather than individual job shifts. It may be, as Palmer suggests, "... that career framework considerations outweigh accidental circumstances if one looks at the record of jobs *over time*, as distinct from a cross-section view of a number

of single job transactions in a local market at any given time" (p. 66). This is only one of many interesting problems which remain to be cleared up by future work.

An introductory essay by Wight Bakke and a concluding note by Gladys Palmer stress the economic and social implications of labor mobility and pose a series of challenging questions. Thus Bakke inquires "... can we undergird the economic and social strength of our nation by adequately and efficiently distributing our labor resources, without destroying, for ... workers and employers, the free choice and free movement which ... have encouraged them to loyalty to the American system?" (p. 7). Palmer expresses an opinion that "in a free labor market the costs of 'too much' mobility can be more readily absorbed than the costs of 'too little' mobility" (p. 112), and supports this by some discussion of the limited mobility in European labor markets. There is clearly a range of issues here to which economic theorists have not yet given adequate attention. What precise meaning(s) should be given to the term "mobility" for purposes of general reasoning? How much mobility, and what kind of mobility, is desirable? What does one mean by "too much" or "too little" mobility? What institutional arrangements in the labor market will be conducive to the amount and kind of mobility we want?

A reviewer must stick to his volume. It is pertinent to note, however, that this book appears almost simultaneously with two others: Gladys Palmer, *Labor Mobility in Six Cities*, and Herbert Parnes, *Research on Labor Mobility*. These volumes, both sponsored and published by the Social Science Research Council, should be regarded as companion pieces to the present work. The three together give one a heady feeling that labor economics is coming of age and that it will in time compare favorably, in point of generality and precision, with the more venerable branches of economic science.

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*Labor Mobility in Six Cities. A Report on the Survey of Patterns and Factors in Labor Mobility, 1940-1950.* By GLADYS L. PALMER, with the assistance of CAROL P. BRAINERD. (New York: Social Science Research Council, 1954. Pp. xiv, 177. \$2.75; paper, \$2.25.)

This volume summarizes the findings of the labor mobility project undertaken by the relevant research centers of seven universities<sup>1</sup> in cooperation with the Committee on Labor Market Research of the Social Science Research Council. The raw data analyzed by these centers were composed of ten-year work histories collected in January and February 1951 by the United States Bureau of the Census from workers in sample households in six cities: Chicago, Los Angeles, New Haven, Philadelphia, St. Paul, and San Francisco.

As might be expected, this project has yielded a wealth of statistical information on what are now fairly standard inquiries in labor mobility research—the volume of movement, the contrasts between mobile and immobile workers in

<sup>1</sup> University of California at Berkeley, University of California at Los Angeles, University of Chicago, Massachusetts Institute of Technology, University of Minnesota, University of Pennsylvania, and Yale University.

terms of age, sex, etc., the industrial and occupational direction of the movement, and so on. But for the first time such information is available in a form which lends itself to useful comparative analysis. Hitherto it has been very difficult (if not impossible) to compare the findings of labor mobility studies made by individual scholars either because the studies covered different periods or used different research techniques, or both. But for all practical purposes those obstacles have been removed in this project. It should be noted, however, that the project findings do not lend themselves to any "universal" generalizations because of the limited number of cities involved—not to mention other considerations.

In many respects the patterns of mobility are remarkably similar in the markets studied. Thus, "differences in the incidence of mobility among different groups of workers and the kinds of job shifts made follow a similar pattern in different cities, regardless of whether a city's degree of mobility is relatively high or low. Mobile workers differ from immobile workers in the same ways in Los Angeles and San Francisco, which have the highest job mobility, as in New Haven and Philadelphia, which have the lowest." But, as the preceding quotation indicates, there are differences between the cities in the volume of mobility, which are attributable mainly to "the relative importance of migrants in the city populations." And it is interesting to know that "differences in the industrial structure of the economies of the cities . . . do not account for any appreciable differences in mobility."

To say that the findings of this research project are indispensable for any future labor market study is to spell out the obvious. But indispensable as they are, these findings are only a beginning. For they are essentially aggregative statistical data which merely offer us clues in our search for the more fundamental determinants of labor mobility; clues which are necessary, but hardly sufficient, for an integrated understanding of mobility. This is not to imply that these data cannot be used directly for many a practical policy problem; they clearly can. But the scholar must have more. He cannot be satisfied with merely knowing that younger workers are more mobile than older ones, or that the unskilled move more frequently than the skilled, and so on. He must know *why* that is so—and for reasons far more compelling than the satisfaction of idle intellectual curiosity; there are long-run pragmatic considerations involved. Moreover, important mobility differences between individual workers in the same socio-economic group have to be explained adequately; and here the "group approach," even at the analytic level, is insufficient, for we must perforce dig into personality determinants.<sup>2</sup>

This study has relatively little to say about vertical occupational mobility, since the basic data are far too skimpy in that regard. And yet there is a crying need for a thorough exploration of such mobility, granted that it is the cornerstone of stratification theory which, in turn, is crucial for constructive policy decisions of either the private or public variety.

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<sup>2</sup> Cf. S. M. Small, "Unconscious Determinants of Vocational Choice," *New York State Jour. Medicine*, vol. 53, no. 20.

*Research On Labor Mobility: An Appraisal of Research Findings in the United States.* By HERBERT S. PARNES. (New York: Social Science Research Council. 1954. Pp. xi, 205. \$1.75.)

With this publication the Labor Market Research Committee of the Social Science Research Council brings to a temporary close the program for stimulating research in the area of labor mobility which it started in 1946. During the past year, the Council has published, in addition, the findings of the largest empirical study of labor mobility in the United States to date, *Labor Mobility in Six Cities*, and has sponsored a small volume of interpretive essays, *Labor Mobility and Economic Opportunity* (New York, 1954), written by members of the Labor Market Research Committee and reflecting the mature thoughts of this group on the implications of mobility research for present-day society. The three publications together represent a substantial contribution to students and practitioners in this important field.

Within its limits, and as long as it adheres to its limits, Parnes' volume is a first-rate production. With the general advice of the Labor Market Research Committee, the author analyzed the significant American studies of labor mobility conducted by labor economists since the early 1930's. From these studies Parnes attempted (1) to ascertain the basic concepts, definitions, hypotheses, and methods used and the theoretical and methodological problems encountered, (2) to summarize and appraise the findings of these studies as to the extent, character, and determinants of labor mobility, (3) to relate the empirical results to some of the assumptions and conclusions of economic theory on the process of labor allocation, and (4) to suggest areas for fruitful further research. These four objectives have been accomplished in a thoughtful and skillful fashion. The author has not simply taken the research studies at their face value, but has assessed their weaknesses as well as their contributions with a critical eye.

The major limitations of the volume arise partly from the limits deliberately imposed by the author on the scope of his undertaking and partly from the scarcity of empirical work in important segments of the field. The scope of the volume was limited "to labor mobility only in relation to flexibility of the labor supply," and within this area primarily to short-run economic and demographic factors. The labor-demand side is treated briefly and inadequately as a determinant factor. This delimitation of the scope of the field is serious because it has resulted in the neglect of a number of important studies which focus on sociological, psychological, and political considerations. Indeed, the author violates his own well-expressed and sound judgment:

Nor does the fact that the study is confined to what might be regarded as the economic, as opposed to the sociological, implications of mobility suggest that each of these is a distinct domain, not to be trespassed upon by specialists in the other. Since the boundaries that divide the social sciences reflect no comparable compartments in real life, research will probably be most fruitful if investigators roam wherever the specific nature of their problems leads them, so long as their competence assures a firm footing. (p. 3)

The emphasis on short-run economic factors is all the more unfortunate in

view of the SSRC's traditional encouragement of cooperation among the several social sciences.

As long as the study concentrates on "the extent and character of labor mobility," the emphasis on short-run economic factors is not too serious—it results in an incomplete but not necessarily distorted picture. However, once attention is turned to the determinants of labor mobility, whether personal or institutional, the inevitable difficulties implied above emerge. The author recognizes the difficulties but the implementation is unsatisfactory.

The chapter dealing with determinants is further handicapped by the scarcity of available data. Whereas the discussion of the extent and character of labor mobility is based almost entirely on empirical studies, the discussion of institutional determinants (to which half of the chapter is devoted) depends heavily on a few theoretical and impressionistic writings by labor economists. The author is so much at a loss for empirical materials in this connection that he feels obliged to refer to two British writings, although he specifically states at the outset of the volume that he is ignoring non-American contributions.

The chapter which may be of greatest interest to general economists is that concerned with the relevancy of labor mobility studies to certain assumptions and conclusions of the neoclassical theories of labor allocation. Specific attention is paid to four questions bearing on three assumptions and one conclusion of these theories: "(1) the extent of workers' knowledge of job opportunities and the ways in which they go about finding and taking jobs; (2) the criteria by which workers make their job decisions; (3) the extent to which the voluntary movement of workers is in the direction of higher paying firms; (4) whether the mobility of workers appears to be effective in reducing differentials in wages and other terms of employment among comparable jobs" (p. 187).

As is well known, empirical studies indicate that the "real" world is much more complex than the models of the neoclassicists. Parnes' analysis reaffirms this view but indicates somewhat more precisely the directions in which neoclassical theory might be modified in order to produce more realistic and useful models.

Although other minor criticisms may be made with respect to this survey (such as the relative neglect of materials on nonmanufacturing workers, the limited reference to mobility in and out of the labor force, and the failure to consider materials bearing on labor mobility in studies not primarily concerned with the subject), Parnes has made a valuable contribution to the field, not only by pointing out significant developments in knowledge in the past two decades but also by indicating the gaps and inadequacies. Indicative of the rapid progress in the field is the fact that since the appearance of the volume, labor mobility studies have been published, for example, by the Universities of Princeton, Illinois, and Columbia, a large-scale project has been launched at the University of Pennsylvania, and studies have been advanced at several universities and by the SSRC itself in the occupational-choice area.

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*Arbitration of Wages.* By IRVING BERNSTEIN. (Berkeley and Los Angeles: University of California Press. 1954. Pp. x, 125. \$3.50.)

A relatively unknown phase of collective bargaining is the voluntary submission of unresolved wage differences to a neutral third party. Since in peacetime probably less than two per cent of general wage changes are reached in this fashion and since even these are concentrated largely in urban transit or other small firms, this ignorance is no surprise. In the five years here covered in detail, only 209 cases were found.

Nevertheless, as a possible alternative to a strike when negotiations break down over a matter of "a little more or a little less," wage-arbitration is worth this serious study by the former chairman of the Committee on Research of the National Academy of Arbitrators. Not the least useful contribution is the assembling of diverse opinions about the conditions under which such arbitration is likely to succeed.

The main portion of the monograph is devoted to classification and analysis of the cases. Of major interest is the analysis of the grounds stated by the arbitrators as the principal reasons for their decisions: intra-industry and inter-industry comparisons and costs of living, with other arguments usually having little weight. The difficulties in the way of using these criteria (and many others) are fully pointed out. Of course, in some limited classifications the sample is too small to justify generalization.

It is unfortunate that the author did not include an analysis of the arbitrators themselves. In evaluating the principles used it would be helpful to know how frequently certain reasons were advanced by economists or lawyers or others. Many of the quoted decisions were rendered by economists, but one wonders about the many others.

Bernstein has made a twofold contribution: one to general knowledge and the other in providing a ready reference for negotiators considering the possibilities of wage arbitration and unwilling to rely on the limited experience of business or union associates.

H. FABIAN UNDERHILL

*Indiana University*

*Industrial Conflict.* Edited by ARTHUR KORNHAUSER, ROBERT DUBIN and ARTHUR M. ROSS. (New York: McGraw-Hill. 1954. Pp. xi, 551. \$6.00.)

This work is the result of collaboration in both editing and writing on the part of a psychologist, a sociologist, and an economist. It was prepared for the Society for the Psychological Study of Social Issues; but the three authors (each of whom made two separate contributions under his own name, in addition to sharing in two further chapters "by the editors") emphasize the fact that "This volume was conceived as an integrated and genuinely interdisciplinary approach to the issues of industrial conflict in the United States." On the whole, the remaining thirty-odd chapters serve to justify this claim, since they comprise a diversified but clearly classified series of attacks upon the problems of industrial conflict, with the economic aspects by no means underrepresented in relation to the sociological and psychological ones.

Obviously it is impossible, within the confines of a brief review, to analyze

or to mention separately the individual contributions of even a few of the thirty-nine persons who had a hand in this joint effort. Fortunately, however, the material is so well organized and integrated that it lends itself to certain meaningful observations which may serve to indicate what the full text has to offer. This is true partly because the editors have been signally successful in avoiding the poorly coordinated, "jumpy" type of manuscript which so often emerges as the product of group writing and research, and partly because they have provided an enlightening series of interlarding summaries, explanations and interpretations.

The general tone of the work is analytical, probing, skeptical, and sophisticated, rather than doctrinaire, dogmatic or naïve. In countless instances questions are asked rather than answered; and where answers are attempted, they are more likely to be pictured in intermediate greys rather than in absolute blacks or whites, with emphasis upon the qualification that more understanding and insight, as well as more facts, are needed before assured conclusions may be justified.

It is not even assumed that industrial conflict is always and necessarily bad. Whereas economists have been accustomed to thinking of it primarily in terms of overt belligerency, as evidenced particularly in terms of strikes, some sociologists look upon it as a sign of industrial democracy at work, in contrast to others who regard it as evidence of breakdown and disorder; while psychologists may see it as an expression of antagonistic interests and desires, at both the group and individual levels, which may have its therapeutic as well as its dangerous aspects.

But in any case, whether it be a dark cloud with silver linings or without, industrial conflict cannot be allowed to cover the entire horizon. It must be "handled," or curtailed, even though it cannot be (and conceivably should not be) entirely prevented. The major means to this end (coming under the three main categories of procedures for reconciling, compromising or adjudicating controversies, of devices for eliminating the sources of dispute, and of social controls by government) are examined in detail and in a manner which is competent and sometimes challenging but which contains little that is new to knowledgeable students of the field.

It is pointed out, as recent studies have indicated more and more clearly, that the social cost of strikes, taken as a whole, is small enough to be readily tolerable, and thus to justify the continued use of this vital adjunct to collective bargaining. One major exception, the national emergency strike, must necessarily be dealt with by government, but on a basis which allows for a considerable amount of improvisation within a broad framework of indicated expectations. The governmental emergency measures of a democracy in peacetime are inevitably conditioned by public opinion, which seems to be more responsive to direct knowledge and personal identification than to propaganda. By the same token, measures taken by industry to deal with substantive matters such as wages, hours and working conditions are more effective in curtailing conflict than are "communications" measures concerned with changing employee attitudes through morale-building, group-dynamics, or "the human relations approach."

There is repeated emphasis upon the twin facts that trade unionism, in its

pursuit of collective bargaining, has exerted a formalizing and stabilizing effect upon industrial relations, and that its aims and activities have far outrun the relatively narrow objective of merely trying to maximize the earnings of its members.

Although the bulk of the book is concerned with the United States, four brief chapters are devoted to a contrasting examination of the environment of industrial conflict in Sweden, in Soviet Russia, in Nazi Germany, and in the nationalized industries of Great Britain.

ELMO P. HOHMAN

*Northwestern University*

*Industrial Relations and the Government.* By WAYNE L. McNAUGHTON and JOSEPH LAZAR. (New York: McGraw-Hill. 1954. Pp. ix, 531. \$6.00.)

The authors of this book, according to the publisher's announcement, recognize that there is more than law to the study of labor and the government. Consequently, while the book is primarily a text in labor law, it includes discussion of the historical, sociological and economic aspects of the problems.

The main structure of the book follows a labor-law format—law applied to the employment relationship, the rights of employers, the rights of employees, and cooperation. Each subject is treated historically, and "other aspects" are fitted in as well as can be expected.

From a layman's standpoint, the law is handled very well; the style is lively and lucid, without sacrificing detail and thoroughness. The strongest feature of the book is the way it shows how the reasoning in court decisions became more and more tortured under old laws, until new laws were obviously required. Any reader should gain valuable insight into the process which changes institutions to fit the times.

The book incorporates new materials, which however are often variations on an old theme. Labor is said to have emerged from the quasiparadise of the guild system, through a Hell of *laissez faire*, toward a Galbraithian paradise of a perfect balance of power, aided by government service functions, rather than regulation. The authors' debt to the Webbs and to the Commons and Perlman works is evident, both in content and in interpretation.

One aspect of this tradition seems to be the belief that the only enemies of employees are crooks or employers and their agents, often including the courts. This belief, in turn, leads to several unsatisfactory generalizations and assumptions. One is that legislation can be interpreted as a pendulum swinging between prolabor and proemployer sentiments (p. 48, p. 160). Another results in a cynical treatment of very real questions involving the freedom of workers to negotiate their own terms of employment (p. 24). A third results in a rather casual treatment of the labor monopoly problem; in this text, this problem is left up in the air (p. 74).

The authors lend recognition but not much understanding to these problems. This is most evident in the discussion of the Taft-Hartley Act (esp. pp. 154-60). With this Act, the pendulum seems at least to have swung into a new dimension, if the analogy has not broken down altogether. We are now concerned with the protection of individuals' rights from attack by labor organizations, as well as with the balancing of bargaining power. But this recent concern for

rights cannot be (and is not) dismissed as pure hypocrisy, as earlier appeals to workers' freedom were.

Since protecting individuals seems to weaken employee organizations, the authors are left with real dilemmas which they cannot resolve. They believe the pendulum has swung too far; but all we can hope for is "maturity, [with which] the importance of regulatory and enforcement functions of the government will diminish, while . . . mediation, conciliation, and arbitration . . . grow" (p. 160).

In sum, this book is a workmanlike job of presenting and implementing a well-worn approach to labor problems. A new approach would be highly desirable, but this book does not supply it.

PAUL W. COOK, JR.

*University of Chicago*

*The System of Industrial Relations in Great Britain.* Edited by ALLAN FLANDERS and H. A. CLEGG. (Oxford: Basil Blackwell. 1954. Pp. 380. 30 s.)

In this volume of six essays, a group of British scholars has provided a long-needed and comprehensive view of British industrial relations. The authors, all but one on university staffs, stay within the intent of the subtitle, "Its History, Law and Institutions," and integrate historical development with current practice in concise fashion throughout. The stimulating opening chapter "Social Background" by Asa Briggs, an Oxford historian, ranges from the impact of organization on the factory system to the current status of management and human relations. He bases his interpretation largely on three changes "which have influenced the background of relations between employers and workers—the advance, by no means a continuous advance, of unskilled labour; the rise of socialism; and the growth of the welfare state."

Kahn-Freund, lecturer on labor law at the London School of Economics, gives a meticulous exposition of the "Legal Framework" covering the nature of the work contract and the growth of government participation. He does not overstate the importance of the law:

There exists something like an inverse correlation between the practical significance of legal sanctions and the degree to which industrial relations have reached a state of maturity. The legal aspect of those obligations on which labour-management relations rest is, from a practical point of view, least important where industrial relations are developed most satisfactorily. There is, perhaps, no major country in the world in which the law has played a less significant role in the shaping of these relations than in Great Britain and in which today the law and the legal profession have less to do with labour relations. In the writer's opinion this is an indication that these relations are fundamentally healthy.

Kahn-Freund continues with a statement of a philosophy which pervades the entire volume. "Reliance on legislation and on legal sanctions for the enforcement of rights and duties between employers and employees may be a symptom of an actual or impending breakdown and, especially on the side of the unions, frequently a sign of weakness, certainly not a sign of strength."

J. D. M. Bell, a Glasgow historian, concisely discusses trade union structure, government, objectives, and methods. His analysis of trade union purposes

moves beyond the traditional improvement of wages, working conditions and the status of workers and includes "extending the area of social control of the nation's economic life, and participating in that control." H. A. Clegg of Oxford provides a much-needed chapter on the extensive organization of both private and public employers for collective bargaining which contrasts strikingly with developments in the United States. The organizational response of employers to union growth presents unique difficulties to the researcher, and the author makes the book's most valuable contribution in an area relatively unexplored to date.

Clegg also collaborates with T. E. Chester, director of The Acton Society Trust, in the chapter on "Joint Consultation" which, like the Whitley system in government service, is of considerable interest but has no substantial parallel in this country. Allan Flanders, senior lecturer in industrial relations at Oxford, provides a chapter on the evolution and process of collective bargaining as both an economic device and a social institution. Flanders emphasizes the difficulties of applying national agreements at the workshop level and raises the basic question of deciding "what is the concern of society as a whole, what should be settled on an industrial scale, and what is the affair of the employees in a single enterprise or a smaller group within it."

The editors make an unnecessary explanation of the "concentration on formal institutions" in view of the recent advance in the "new school of 'human relations in industry.'" Footnote references provide ample compensation for lack of a bibliography. While, with the possible exception of the chapter on employers, little of the book is devoted to new material, the authors have, within the scope of a single volume, produced an extremely well-written and thorough introduction to their subject—probably the best text available in the field.

HARRY STARK

*Rutgers University*

*Industrial Pensions.* By CHARLES L. DEARING. (Washington: Brookings Institution. 1954. Pp. x, 310. \$3.75.)

Not only in the public thought of the day but also among directly interested groups of employers and organized labor there is much unclear thinking in the matter of industrial pensions. This controversial problem sounds simple when approached in terms of the generalities of labor organizations, and even of governmental agencies. Among the latter are the conclusion that social insurance and pensions should be among the primary charges upon the employer's income, that industrial pension and other welfare schemes are conditions of employment and must be bargained for. Powerful labor groups assert that unilateral employer contributions "belong to the workers," that benefits should be based on need rather than earnings, and that the entire cost burden should be placed upon employers as a means of forcing them to bring about a more adequate federal old age and survivors' insurance program. Dearing has plunged into the heart of this controversial field, and, in a thoroughly scientific fashion has succeeded in clarifying the issues so that they should be understandable to the layman, to employers and to labor leaders alike.

There is presented an historical picture of the pension movement prior to the depression of the 1930's, the effect of the depression in bringing about contributory pension plans with vested rights for employees, and finally the wartime and postwar stimulation of pension plans in the form of employer-financed schemes with little or no vesting of rights for workers short of retirement. The influence of the National Labor Relations Board's interpretation of the law in 1948, and the subsequent Circuit Court of Appeals decision in 1949 setting forth the principle that no employer can initiate, terminate or alter any welfare program, whether contributory or noncontributory, voluntary or compulsory, without collective bargaining are shown to have forced a hasty development of industrial pensions which in many respects are not in the interest of the worker, the employer or the public.

The analysis of the structure and operation of various types of pension plans is invaluable. Dearing, basing his figures on good employment and favorable factors, anticipates by 1960 a participation by about 22 million workers, or approximately two-thirds of the work force which would in any way be eligible for coverage by private pension systems. As to contributions, the author states "In the long run the true economic cost of maintaining a pension system will be determined by the extent to which the claims of future beneficiaries for goods and services can be satisfied out of the enhanced productivity generated by the productive employment of accumulated pension funds." Following an analysis of the amount and nature of industrial pension contributions in the various trade and industry groups, Dearing estimates the annual aggregate contribution needed to support the plans in 1950 at something over \$2 billion. Making adjustments for increased coverage, the extension of funding for past services, liberalization of benefits, and a moderate allowance for an increased cost of living, the author reaches a projected payment by companies and employees of about \$6.6 billion annually by 1960. He calculates new money savings (including pension contributions) at from \$18 to \$22 billions annually, and outlets for new capital at \$17 to \$18 billions, suggesting possible investment problems in the 1960's.

Particularly illuminating for persons directly or indirectly involved with industrial pensions as employers or potential beneficiaries, are the author's appraisal and critique of industrial pension plans, and his discussion of the allocation of responsibility for pension financing. It is pointed out that since not more than 32 per cent of the total labor force can expect coverage, industrial pensions are capable of performing no more than a supplementary role in solving the national problem of old-age security. Most of those covered by existing pension plans must remain with a single employer throughout the major portion of their working life and to normal retirement age in order to realize benefits. The net effect of mandatory collective bargaining, asserts Dearing, has been to freeze inherent defects into the expanding pattern of pension development. Also, the incorporation of need as the basis for pension eligibility by powerful union groups violates the concept of pension contributions as deferred wages, and results in calling upon workers in the higher earning brackets to subsidize the low-wage earners.

The allocation among government, management and individual workers of direct responsibility for provision for old age security should be conditioned by economic standards and social criteria consistent with a free enterprise system.

Sound public policy would seem to place the prime responsibility on the individual, whether operating unilaterally or collectively, for providing the level of retirement income desired beyond that of the federal program. Nevertheless, there is a valid role for industrial management, Dearing states, through pension programs adapted to the operating policies, financial capacity and competitive position of individual corporations.

EARL E. MUNTZ

*New York University*

*Wildcat Strike.* By ALVIN W. GOULDNER. (Yellow Springs, Ohio: Antioch Press. 1954. Pp. 179. \$3.00.)

The author and his research team had the good luck to be able to study firsthand a wildcat strike from beginning to end in a small (225 employees) gypsum plant. Other research in this plant is reported in the same author's companion volume, *Patterns of Industrial Bureaucracy*.

The author has three objectives: to describe a wildcat strike in detail; to explain why and how it happened; and to develop the "rudiments of a general theory of group tensions." Each objective is accomplished with clarity and perception. Primarily sociological in orientation the study contains material of practical moment for union and management representatives and of factual and academic interest for students of industrial relations and group tensions in several disciplines.

The description and explanation of the strike make up two-thirds of the book. A seemingly stable labor relationship based on close worker-supervisor relations and on fulfillment of worker expectations—an "indulgency pattern"—was disrupted by changes stemming mainly from the economics of postwar competition. Technological innovations, a change of plant managers, and replacements among middle management brought a circular process of tightening supervision, violation of worker indulgency expectations, loss of worker motivation to work and obey, and worker aggressiveness. In the resulting tension an inadequate grievance machinery and a leadership on both sides preoccupied with status and market considerations were incapable of preventing the strike. It was settled by an agreement which safeguarded the status interests of union and company leadership by "bureaucratizing" their relationship without resolving underlying causes of tension. The experienced observer of industrial relations will find little that is analytically new in this part of the study, but the picture of the wildcat strike is unusual for fullness of detail, authenticity, and sureness of understanding.

In the rest of the book the author attempts to generalize the findings of this case study into principles of a general theory of group tensions. He codifies into seventeen general propositions the factors which create tension in social systems; lists seven different "disorganization patterns" into which these tensions arrange themselves; and distinguishes three different patterns of "defenses" or responses to tensions. Some of these principles seem obvious truisms clothed pretentiously, but all ring true, and many provide genuine insight toward the author's goal of erecting a bridge between pure and applied sociology.

VAN D. KENNEDY

*University of California, Berkeley*

### Population; Social Welfare and Living Standards

*World Population and Production: Trends and Outlook.* By W. S. WOYTINSKY and E. S. WOYTINSKY. (New York: Twentieth Century Fund, 1953. Pp. lxii, 1268. \$12.00.)

Political arithmetic in the twentieth century is a very different art from that in the seventeenth. The statistical brickmakers, who were formerly hard put to it to find straw, are now embarrassed by an excess of it. Doubtless its quality varies and the problems of selection are more difficult than ever before. Nevertheless the movement from scarcity to abundance is the most outstanding characteristic in this field of human endeavor.

All this is reflected in the 1,268 closely printed pages of this monumental effort of the Woytinskys. It is a work in the tradition, not of mere compilers of statistical tables, but rather of that different species who are inspired by curiosity to probe both quantitatively and qualitatively into the multitudinous activities of man. It is not easy to place it by comparison with other works. It differs from the *Statesman's Year Book* in being organized according to subject-matter and not countries, and in attempting interpretation and tracing antecedents. At some points it may seem to resemble Zimmerman's *World Resources and Industries* but its attempts at interpretation are less ambitious, its range of topics somewhat wider, and its preoccupation with measurement greater. It is rather more quantitative and at some points less detailed in its descriptions than Chisholm's *Handbook of Commercial Geography* (rewritten by Stamp).

It is not surprising that a reference work of this type should have appeared at a time when the volume of statistical material and the extent of specialization are rapidly expanding and only a few professional scholars can hope to follow the numerous and widely scattered publications which reflect this expansion. The present work will undoubtedly be of great value to a wide range of readers, both professional and lay. It is a ready reference, not only to facts but also on some subjects to quantitative methods and even historical origins. The industry that has gone into the work is staggering and if Congressional probers of alleged waste in government belonged to the reading section of the public, they might be expected to point to the fact that two persons have accomplished a task of such magnitude as might have engaged the services of a whole bureau!

It is in no sense a detraction from the Woytinskys' accomplishment to point to a few shortcomings, some of them inevitable in a work of this scope. First, there is the difficulty in keeping such a work up to date. It could presumably be met only by issuing periodic supplements, which, after a time, would grow cumbersome. The size and expense of publication may be a deterrent to new editions: a more modest form of publication, with sparser illustration might possibly have been better adapted to survival.

Next, the range of qualitative material carries with it dangers of lapsing into superficiality at some points. The Woytinskys appear on the whole to have come out well in this respect. No single reviewer can hope to judge the adequacy of all interpretations and selections in so wide-ranging a work. Space

will not permit a detailed examination and two examples only will be discussed here.

First the Huntington theory of the relation of climate to energy is described (p. 29) uncritically without reference to the severe criticisms to which his methods have been subjected and to which he never made an effective reply. Recent studies in the relation of climate to man and animals are not touched on in the text or bibliography.

Second, it is stated (p. 139) that "... the net reproduction rate measures the vitality of the population in terms of the *long range* trend, rather than current changes." Also, "A net reproduction rate of 1.0 indicates a state of demographic equilibrium in the long run." These statements may seem plausible, but I do not think I should be alone among population students in regarding them as dangerous and misleading. Not even a trend line calculated empirically to fit a series of annual net reproduction rates, nor even a moving average, let alone the rate for a single year, could claim such properties. No reference is given in the bibliography to the work of Hjanal, Karmel and others in recent years on the net reproduction rate.

In neither of these two examples do I wish to suggest that the authors were obligated to survey the particular points in question in exhaustive detail. But it does seem justifiable to suggest that they would have been better advised to have been more careful in selecting the theories which they described and to have avoided some of the categorical statements which they made. Their discussion on the points in question should have contained either more or less than it did.

These, however, are only points of detail and the feeling with which one closes the work can only be one of admiration for the immense task which the authors have carried to a successful conclusion.

E. F. PENROSE

*The Johns Hopkins University*

*Workmen's Compensation.* By HERMAN M. SOMERS and ANNE R. SOMERS. (New York: John Wiley & Sons. 1954. Pp. xv, 341. \$6.00.)

Students of social security have long complained that workmen's compensation, the oldest of our social insurance programs, is the one about which least is known. The standard studies of workmen's compensation administration were written between 1924 and 1940: the most recent comprehensive technical book, Larson's *The Law of Workmen's Compensation*, is, as its title suggests, written primarily for lawyers, and the only study which frankly aimed to appraise the achievements of the system by reference to its social objectives, namely Reede's *Adequacy of Workmen's Compensation*, was based on data as of 1940. In contrast to our extensive knowledge of the functioning of other social insurance programs, we have had to be content in the field of workmen's compensation with estimates of total expenditures and until very recently no one had even ventured to estimate the total number of beneficiaries.

It must be admitted that the effort to assess the present system demands great fortitude on the part of the investigator. The many different jurisdictions, the diversity and complexity of the laws, statutory and administrative, the variety

of types and standards of administration, the involvement of private insurance and the shocking paucity of officially released data, coupled with the wide-ranging impact of workmen's compensation legislation which requires the student to delve into the technical literature of law, insurance, medicine and industrial relations probably explain why the field has been so long neglected.

Fortunately Professor and Mrs. Somers have not allowed these obstacles to deter them. They have produced a study which is remarkable not only for the completeness of its coverage and breadth of treatment, but also for the exhaustiveness of the sources and authorities consulted. At long last we are in a position to know what workmen's compensation is like.

The findings reported by the Somers (with a pungency of style that is refreshing in so technical a work) confirm one's worst suspicions. The program has not only failed to keep pace with the changes that have occurred in the economic and social environment since it was inaugurated: it is not even meeting the test of its own original objectives. The annual cost of workmen's compensation is now about \$1.3 billions, or about the same as unemployment compensation. Yet neither employers nor workers pay anything like the same attention to its achievements and administration, while the public interest is in effect zero. Of this large sum, only about half goes to workers in the form of benefits: the remainder is consumed by overhead costs, primarily those of insurance but also legal fees and administration. Employers have not reaped the alleged advantages of financing through competitive private insurance carriers because uniform manual rates have been established with expense loadings that reflect the experience of the less efficient carriers (some 200 carriers, many small and inefficient, share in the business) while the more efficient make large profits. Nor do employers enjoy the economies of rates based on national experience. Despite original hopes, the administration is almost incredibly litigious (claimants' legal costs consume between 3.5 and 16.6 per cent of the cash benefits in the different jurisdictions) and the authors maintain that such advances as have been made over the years have come at the expense of ever-increasing complexity, ambiguity, litigiousness and costliness.

In fact, apart from some growth in medical benefits, the advances are difficult to discern. The fixed dollar maximums and the limits to duration have resulted in payments that today cover little more than one-third of wage loss, while for the permanently injured and survivors of deceased workers the percentage is around 20. Although expenditures on medical care have increased, the medical program has grave deficiencies and is subject to little supervision or qualitative control. Rehabilitation, which might have been expected to be an area in which workmen's compensation would excel, has been disappointing, with the notable exception of the programs of one or two insurance companies. Indeed, the chapter on rehabilitation is perhaps the most dismal of all for it is a record of missed opportunity. The state administrations have in general been starved of appropriations, and salaries are disgracefully low in view of the highly technical nature of the work. Officials have been relatively helpless in the face of the powerful insurance and medical groups whom in theory they should supervise and control. Even in regard to prevention, the much-advertized achievements of workmen's compensation are, the Somers suggest, largely

in the past: other influences today far surpass those exerted by the workmen's compensation system.

What then is the future of this once important social security program? It is here, perhaps, that the authors are least satisfactory. They appear to place their faith in the further development of the already substantial number of supplementary security systems, some public, some private, which in total somewhat mitigate the deficiencies of workmen's compensation. But as they admit, this complex series of measures involves much overlapping and considerable duplication of payments while offering no assurance that all cases of need for income or medical care will be provided for. Moreover, this duplicating series of programs is costly, and while it may be true, as some European observers hold, that America is rich enough to afford a wasteful social security system, it is difficult not to think that sooner or later both employers and workers will begin to ask whether they could not get better value for the many premiums and social security taxes they pay. The Somers recognize that the future of workmen's compensation will be greatly affected by what action the nation takes in regard to temporary and permanent disability insurance and the socialization of the costs of medical care through social insurance or otherwise. But one would have welcomed a more specific indication of the precise role of workmen's compensation on the basis of alternative hypotheses as to the trend of social security legislation in general. For the question that is left in the reader's mind is whether workmen's compensation is worth salvaging.

But although the authors refrain from indicating the long-run solutions they would prefer, their study will rank as one of the most important books that has appeared in the field of social security for many years. It fills in an admirable way a major gap in our knowledge of social security institutions that had too long existed. And now that the inadequacies and inefficiencies of workmen's compensation have been so fully documented, the book will surely act as a powerful stimulus to a reconsideration of the role of this program, and, so long as it continues in existence, to legislative and administrative reforms.

EVELINE M. BURNS

*New York School of Social Work, Columbia University*

*Our Needy Aged: A California Study of a National Problem.* By FLOYD A. BOND, RAY E. BABER, JOHN H. VIEG, LOUIS B. PERRY, ALVIN H. SCAFF, and LUTHER J. LEE, JR. (New York: Henry Holt. 1954. Pp. xxx, 401. \$4.50.)

Old Age Assistance (OAA) receives heavy financial support from the federal government. It is administered by state and local governments and consequently the magnitude, benefits and eligibility requirements vary widely among the states. California's program is in many ways distinctive: it is the most ambitious, the most publicized, and the most controversial.

This first full-scale study of the California program by a team of Pomona College social scientists will command wide attention because of its unhesitating recommendations with respect to all of the controversial features of the program.

The study is comprehensive. Its ten chapters concern: the socio-economic

characteristics of California's aged; the historical and political background of the present OAA program; the substantive provisions of the California law, its differences from that of other states, and the administrative experience under it; the costs of OAA in California and nationally; attitudes and opinions of the aged and of the county administrators toward the program; and, finally, a set of recommendations.

The major contribution of the study is the wealth of material newly developed by a special 1952 survey of 890 of California's noninstitutional aged, of which roughly one-third received assistance. Based upon this survey (and other sources) a number of findings emerge, among them: (1) California has more people on OAA and more aggregate expense than any other state. (2) California's aged are typically much better off than those in other states, and OAA is largely responsible. (Whether this indicates merely that the program is more nearly adequate, or whether it indicates extravagance is an important question that is not analyzed.) (3) Despite its relative attractiveness, OAA has not drawn a disproportionately large group of indigent aged into the state. (4) Large increases in the size of maximum grants (from \$35 to \$80 since 1935) have been partially reflected in real terms (from \$35 to \$41) over the same period.

Usefulness of some of the data presented is impaired by insufficient attention to questions of statistical significance (the methodological appendix is not adequate in this respect), and by the unfortunately limited amount of cross-classification of the data that is, I imagine, a consequence of the smallness of the sample. Notwithstanding, the data are useful, and it is regrettable that the basic data are not fully presented in an appendix.

The attitudinal data are interesting, but the biases of the respondents are insufficiently considered for the attitudes to have the evidential value that they are later given. Majority opinions of the present group of age toward eligibility requirements quite naturally reflect the majority characteristics of the group with respect to age, citizenship, length of residence, and so on. The bias of county administrators is to keep their administrative burden in hand ("We would be swamped" if the requirement [of citizenship] were removed. . . ." [p. 308]). A further difficulty is the failure to analyze opinions in terms of key variables: "The reported ability of children to help support their parents varies with the parents' educational level" (p. 298)—and, I suspect, with children's income.

Among the controversial recommendations concerning eligibility for OAA are retention of the citizenship requirement, more stringent property limitations, and retention and strengthening of relatives' responsibility. Also recommended are provisions for reimbursement to the state from the estates of recipients, and for a continuation of county (rather than state) administration.

To understand the recommendations it is necessary to recognize an implicit and unestablished premise that virtually preoccupies the authors: the cost of the program is in imminent danger of becoming *too high*. This premise might be justified if either the burden of the program is (or promises to become) oppressive, or if the level of payments to individuals is extravagant. Despite an occasional disclaimer the former is the apparent fear of the authors. The cost in 1953 of the program was \$226 million, of which about half was paid by the

federal government. The reasons given for thinking this is excessive are: (1) this amount is about 9 per cent of the total expenditures of the State and local governments (almost as much as is spent for highways and bridges), (2) it amounts to about \$20 per capita, (3) it is high relative to expenditures in any other state. This evidence is not sufficient, and the issue is too central to be resolved by assumption.

Disagreement with some of the recommendations is beside the point. The point is that most of the recommendations do not emerge from the evidence of the study. Consider the citizenship requirements, which are recommended for the following six reasons (pp. 347-49): (1) three-fifths of the county directors and four-fifths of all the aged in the survey favored it, (2) the proximity of California to the Mexican border, (3) the availability of general relief to those not on OAA, (4) the process of becoming a citizen is not a very difficult one, (5) dropping the requirement would make enforcement of the immigration laws more difficult, and (6) the alternative would cost an additional \$35 million to a state which is already spending more than twice the amount of any other state. Of these points only the first, which is irrelevant, and the last, the significance of which is unestablished, emerge from the study.

Relatives' responsibility is endorsed "because it is sound in principle, both morally and economically" (p. 352). Most surprising is the recommendation concerning county administration. The showing in previous chapters of unequal burdens on the counties, of uneven administration among counties, and related findings led, I thought, to the opposite conclusion. The chief explanation is the authors' belief that "local and State governments have both suffered in recent decades from the transfer of some of their functions to higher levels" (p. 367).

The authors are entitled to their judgments, which neither enhance nor detract from their substantial substantive findings. My chief concern is that these opinions will be given undue credence because they are found at the end of a long book.

PETER O. STEINER

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*Contributions of Survey Methods to Economics.* Edited by LAWRENCE R. KLEIN. With papers by GEORGE KATONA, LAWRENCE R. KLEIN, JOHN B. LANSING, JAMES N. MORGAN, Survey Research Center, University of Michigan. (New York: Columbia University Press. 1954. Pp. ix, 269. \$5.00.)

This is not a book for purists. It is the record of a program of research in process of development, and a composite portrait of a new school of believers in an interdisciplinary approach to economics. The aim of this school is to wed economics and psychology through the method of direct surveys. If the concepts employed seem at times somewhat forced or fuzzy, we must remember that the first steps in a new field are necessarily halting and sometimes misguided. If technical machinery is wrenched into services it was never designed to perform, we must respect the will to get on with the work at hand rather than be content with blueprints for perfection. If emphasis on the scientific character of the enterprise seems rather too insistent and at times pedantic, we must recognize the soundness of the inspiration and respect the ideal aimed

at. And, finally, if the book shows flashes of real genius and contains analysis of the first rank, we must credit these to the vitality of a sound idea in attracting exceptional talent.

The book opens with a long and informative chapter by J. B. Lansing on the concept of consuming unit and the accounting framework used to record consumers' economic behavior. Next is a chapter by George Katona outlining a theory of consumer behavior in which psychological and institutional elements are prominent, and to which the results of re-interview surveys are shown to lend support. There follow two chapters by J. N. Morgan on the determinants of consumer saving; these are similar in theoretical inspiration to Katona's chapter, but they are distinguished by the use of a "net worth" concept of saving (which includes spending on durable consumer goods) and by truly heroic efforts to employ the tools of "analysis of variance." Nearly all practicing statisticians do this sort of thing, and hardly anyone likes the other fellow's compromises; but, on balance, Morgan seems not to claim to have proved too much. The final substantive chapter, by L. R. Klein, adduces an econometric model of consumers' saving behavior which incorporates most of the variables—economic, demographic, and attitudinal—that Katona and Morgan find to be influential. The model provides an interesting case of nonlinear relationships among economics variables, and leads to an important qualification of the relationship between real wealth and spending that has figured so prominently in monetary and fiscal theory. The book is introduced by an apt summary of contents and closes with some thoughtful comments on the role of survey methods in business cycle research—both by the editor.

The book is a contribution on several levels. It adds to our knowledge of consumer behavior. Its structure recapitulates the stages—sometimes groping, sometimes inspired, but generally gaining in authority—through which a program of empirical research usually passes; and it will therefore give depth and realism to discussions of research methodology. But best of all, it teaches us something about the Survey Research Center. Here is an organization with an unrivaled body of data on consumer behavior that has not been made generally available to scholars. Like high explosive, it is to be handled only by experts, which usually means that mixed breed psycho-socio-economist. Now we are given an account of the tricky stuff, are told some of the reasons it is difficult to handle, and are shown some of the expedients by which it can be made to yield up its lessons. One gains the impression that the stuff is in competent hands, but neither the difficulties nor the expedients seem half so esoteric as we were led to expect. There may even be simple economists among us who feel that the stuff should be declassified.

MILLARD HASTAY

*National Bureau of Economic Research*

*Consumer Behavior: The Dynamics of Consumer Reaction.* Edited by LINCOLN H. CLARK. (New York: New York University Press. 1954. Pp. viii, 128. \$4.00.)

This handsomely printed volume is the first issue of a proposed annual pub-

lication, the product of a new Committee for Research on Consumer Attitudes and Behavior, based at Ann Arbor, Michigan. Active leaders include members of the Survey Research Center, University of Michigan, but the participants have included representatives from several universities and several disciplines, a number of market-research agencies, and others from the nonacademic world. Consumers Union has contributed financial support.

The exploratory studies here presented range "over a variety of problems connected with consumer decision and choice." George Katona and Eva Mueller, joint authors of *Consumer Attitudes and Demand, 1950-1952* (1953), present two thoughtful papers on purchase decisions—"a central topic of psychological economics"—with special reference to the research design (Katona) and the sample survey (Mueller). They are mainly concerned with "the kind and extent of deliberation that accompanies the purchase process," and with "the specific conditions that give rise to careful deliberation."

Ruby T. Norris reports on a field study of house purchases in the New London (Connecticut) area between mid-August 1952 and mid-April 1953—on reasons for buying, alternatives considered, how the house bought was located, how intensively it was looked for, and what the buyers wanted but did not get. The results bring out the wide diversity of buyers' attitudes, divergencies between volunteered opinions and responses to leading questions, and differences between the buyers' judgments and those of the interviewers.

The other two major papers, concerned with living as well as with buying and consuming, are stimulating, even provocative. William H. Whyte, Jr., of *Fortune's* editorial staff, illuminatingly analyzes "the new suburbia" as represented by Park Forest (near Chicago), a "filiarchal" "city of permanent impermanence" displaying "extraordinary homogeneity" and an increasingly communal way of life. Nelson M. Foote explores "The Autonomy of the Consumer," by which he means "the consumer's self-determined use of his sovereignty, his utilization of the opportunity to create his own style of spending." He finds this autonomy worth "cultivating" by the researcher and his employer. But he mainly presents ten challenging research hypotheses relating specifically to what he considers the "new era" that Americans have entered—in which real income will rise chiefly through technical improvements without exertion or self-denial.

A final group of brief papers treats of technical problems in measuring consumer preferences. The Appendix contains a statement about the Committee, summary minutes of its first two annual conferences held in September 1952 and 1953, a subcommittee report on research program, and a short Bibliography on Consumer Behavior.

Economists are increasingly recognizing the importance of consumption in the United States and other advanced economies. Though it has been studied from many standpoints, consumption has so many aspects that our understanding of it is all too superficial and inadequate. Now the American consumption level has risen far above that of 1929, which W. H. Lough discussed in his *High-Level Consumption . . .* (1935), and there is serious talk of "doubling our standard of living" in 25 years or so. Whatever their individual bias, economists of many specialties should therefore welcome the contributions of this

new interdisciplinary body seeking to promote empirical studies of consumer behavior with a view to better understanding of the future.

JOSEPH S. DAVIS

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### Unclassified

*Economics in General Education*. Edited by JAMES GEMMELL, SEYMOUR HARRIS, and S. P. MCCUTCHEN. (New York: Joint Council on Economic Education. 1954. Pp. 151.)

In late summer, 1954, the Joint Council on Economic Education sponsored a conference of economists, educators and administrators of schools of education to discuss the role of economics in general education. The proceedings of this conference have been published under the title given above.

The Joint Council on Economic Education is "a non-profit educational organization created to assist school systems and teacher educating institutions in improving the quality of social and economic education through curriculum research, workshops, seminars, in-service education programs and the preparation of materials for teachers and pupils" (cover page). From the time of its organization in 1948, the Council has carried on a varied program of activities, including a sponsoring role (in cooperation with various colleges and universities) in some 120 workshops in economic education for public school teachers.

The publication here under review is made up of summaries of various papers presented or developed at last summer's conference. The Part I papers deal with such general issues as the importance of economic understanding in a free society (Roy Price, Syracuse University), the nature of economic understanding (Ben Lewis, Oberlin College), the main steps in economic analysis and the relationship of analysis and policy (Seymour Harris, Harvard University), and major issues in economic education (Lewis Wagner, Iowa State).

The Part II papers deal with the organization of college courses in economics. The first is a committee report on the terminal course in economics, including an analysis of the aims and organization of a terminal course and a brief review of several existing or proposed course organizations. The second is a report of a similar study of the interdisciplinary course.

The Part III papers deal with suggestions for the treatment of particular topics in the secondary schools. The first presents a suggested frame of reference for the teaching of economics in the secondary schools (Arno Bellack, Teachers College, Columbia University). The second is an outline of how the functioning of the economy might be presented, using the problem of unemployment as a point of departure (E. T. Weiler, Purdue University, and Glenn Ogle, director of the St. Louis Regional Council on Economic Education). The third is similar to the second, except that the point of departure is agriculture (James Calderwood, Committee for Economic Development). The fourth follows the same pattern, with the consumer as the point of departure (Persia Campbell, Queens College). The fifth paper is a brief description of an historical approach to a study of inflation (Seymour Harris). The sixth is a suggested technique for developing (through class participation) an under-

standing of the problems involved in maintaining economic stability (Lawrence Senesh, Joint Council on Economic Education). The final paper in this section presents an illustration of what a teacher must know if he is to do an effective job of conveying an understanding of the issues in the area of income distribution (Lewis Wagner and C. C. Trillingham, Superintendent, Los Angeles County Schools). Part IV contains but one paper, a committee report on the general subject of needed research in economic education.

I would recommend a reading of this publication to all college teachers of economics, particularly to those who have had no contact with the workshops or other activities of the Joint Council. These papers contain no definitive expositions of theory or teaching technique but they do convey the "flavor" of this program in economic education. To those who believe that it is sufficient to develop a small, well-trained cadre of professional economists, to those who question the value of a "little knowledge" in economics, to those who are unconcerned by the growing problem of communication between the professional economist and the layman (including perhaps his own students), to those who look on the professional educators as a hostile, anti-intellectual force in education, these papers present a real challenge. Whether the goals and methods of the Joint Council be right or wrong, here is a group of men trying to do something in economic education which may well be of unique importance. This being the case, an attitude of rigid isolation on the part of most professional economists hardly seems to be appropriate.

BENJAMIN A. ROGGE

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## TITLES OF NEW BOOKS

*Editor's Note:* The following list includes titles of French, Spanish and Dutch publications which have been supplied, respectively, by Jean-Marcel Jeanneney of the Fondation Nationale des Sciences Politiques, Paris, Victor L. Urquidí, Director of *El Trimestre Económico*, Mexico, D.F., and P. J. Verdoorn, of the Centraal Planbureau, the Hague, correspondents of the *Review*.

### Economic Theory; General Economics

ALLAIS, M. *Les fondements comptables de la macroéconomie—les équations comptables entre quantités globales et leurs applications*. Ouvrage publié avec le concours du Centre National de la Recherche Scientifique. (Paris: Presses Univ. de France. 1954. Pp. 91. Fr. 900.)

BARKER, C. A. *Henry George*. (New York: Oxford Univ. Press. 1955. Pp. xvii, 696. \$9.50.)

CLAY, H., ed. *The inter-war years and other papers. A selection from the writings of Hubert Douglas Henderson*. (New York: Oxford Univ. Press. 1955. Pp. xxviii, 445. \$6.75.)

CONGARD, R. P. *La demande et le monopole*. (Paris: Sté. d'Edit. d'Enseignement Supérieur. 1954. Pp. 384. Fr. 2200.)

FISHER, I. *The theory of interest—as determined by impatience to spend income and opportunity to invest it*. Reprints of Economic Classics. (New York: Kelley and Millman. 1954. Pp. xxi, 566. \$8.50.)

FOWLER, C. B., GRIFFIN, J. I., COHEN, J. B., CROUSEY, J., GREENWALD, W. I., and SETHUR, F. *Economic handbook—a visual survey*. (New York: Thomas Y. Crowell. 1955. Pp. ix, 246. \$2.45.)

A graphical presentation of sixty aspects of the economy, accompanied by statistical tables, short descriptive comments, and questions—designed for supplementary use in the basic course.

FRIEDRICH, C. J. and GALBRAITH, J. K., ed. *Public policy—a yearbook of the Graduate School of Public Administration, Harvard University*. Vol. V. (Cambridge: Grad. School Pub. Admin., Harvard Univ. 1954. Pp. vii, 420. \$5.)

FRITSCH, B. *Die Geld- und Kredittheorie von Karl Marx—Eine Darstellung und kritische Würdigung*. Staatswissenschaftliche stud. 17. (Zurich: Polygraphischer Verlag. 1954. Fr. 16.65.)

GALBRAITH, J. K. *Economics and the art of controversy*. (New Brunswick: Rutgers Univ. Press. 1955. Pp. v, 111. \$2.50.)

GHOSH, S. K. *Trends in economic thinking*. (Calcutta: Author, 15 Mahendra Road. 1953. Pp. 57. Rs. 2/-.)

GLANDORFF, M. *Théorie générale de la valeur et ses applications en esthétique et en économie*. Inst. de Sociologie, Solvay Collection Soc. Gen. et Phil. Soc. (Brussels: Les Editions du Parthenon S.P.R.L. 1954. Pp. 324.)

HARRIS, S. E. *John Maynard Keynes—economist and policy maker*. (New York: Charles Scribner's Sons. 1955. Pp. xiv, 234. \$3.)

HARWOOD, E. C. *Reconstruction of economics*. Including papers by M. Brodbeck and R. S. Rudner. (Great Barrington, Mass.: Am. Institute for Econ. Research. 1955. Pp. 110. \$1.)

The principal author suggests in his introduction that a more appropriate title might be something like the following: "What, in principle, is wrong with economics as it is taught and practised today; what should be done about the situation; how might men

be trained to do the job; and, what aspects of the task should have their first attention?"

Apart from three book reviews, which were published earlier by the same Institute, and apart from a statement of the Institute program, the essays are essentially concerned with the philosophy and methodology of the social sciences, and economics in particular.

JAMES, É. *Histoire de la pensée économique aux XX<sup>e</sup> siècle*. Vol. II, *Après la 'Théorie générale' de J. M. Keynes (1936)*. (Paris: Presses Univ. de France. 1955. Pp. 403. Fr. 1200.)

A supplement to this volume *Annexe aux problèmes d'économie internationale* by J. Meiller (59 pp.) has also been published by Presses Univ. de France.

JOHNSON, A. *Economic theory*. (London: Frederick Muller. 1954. Pp. 144. 8s., 6d.)

MAVERICK, L. A. *Productivity—a critique of current usage*. (Carbondale, Ill.: Lewis A. Maverick. 1955. Pp. 30. 50¢.)

MENDES-FRANCE, P. and ARDANT, G. *La science économique et l'action*. (Paris: Julliard. 1954. Pp. 256. Fr. 600.)

MENICONI, F. *Theorie des Einkommens*. Staatswissenschaftliche stud. 18. (Zurich: Polygraphischer Verlag. 1954. Pp. 328. Fr. 19.60.)

PAPI, G. U., ed. *Teoria e politica dello sviluppo economico*. Istituto di Economia e Finanza pub. (Milan: A. Giuffrè. 1954. Pp. 567. L. 3000.)

PAULSEN, A. *Neue Wirtschaftslehre. Einführung in die Wirtschaftstheorie von John Maynard Keynes und die Wirtschaftspolitik der Vollbeschäftigung*. 3d ed. (Berlin and Frankfurt aM.: Verlag Franz Vahlen GmbH. 1954. Pp. xi, 387. DM 18.80.)

REYNAUD, P.-L. *La psychologie économique*. (Paris: M. Rivière. 1954. Pp. 260. Fr. 700.)

ROTHSCHILD, K. W. *The theory of wages*. (New York: Macmillan. Oxford: Basil Blackwell. 1954. Pp. viii, 178. \$2.75.)

SCHOUTEN, D. B. J. *De dynamiek van het kapitalisme* (The dynamics of capitalism). (Leiden: Stenfert Kroese. 1954. Pp. 27. f. 1.50.)

SERAPHIM, H.-J. *Theorie der allgemeinen Volkswirtschaftspolitik*. (Göttingen: Vandenhoeck and Ruprecht. 1955. Pp. 351. DM 33.00.)

SMITHIES, A., and others. *Economics and public policy*. Brookings Lectures, 1954. (Washington: Brookings Institution. 1955. Pp. vii, 137. \$2.)

Arthur Smithies, "Economic Welfare and Policy," J. J. Spengler, "From Theory to Public Policy," F. H. Knight, "Economic Objectives in a Changing World," John Jewkes, "The Economist and Economic Change," Jacob Viner, "International Trade Theory and Its Present Day Relevance," and Lionel Robbins, "Freedom and Order."

SOLO, R. A., ed. *Economics and the public interest*. (New Brunswick: Rutgers Univ. Press. 1955. Pp. xiv, 318. \$5.75.)

STARK, W., ed. *Jeremy Bentham's economic writings*. Vol. III. (London: Allen and Unwin. New York: Burt Franklin. 1954. Pp. 600. 45s.)

VILLEY, D. *Petite histoire des grandes doctrines économiques*. (Paris: Lib. de Médecis. 1954. Pp. 304. Fr. 600.)

WEBER, W. *Wirtschaftswissenschaft von heute—Ein Überblick über moderne ökonomische Forschungen*. (Wien: Springer Verlag. 1953. Pp. 218. \$2.90.)

*International economic papers*, no. 4. Translations prepared for the Internat. Econ. Assoc. Edited by A. T. Peacock, W. F. Stolper, R. Turvey, and E. Henderson. (New York and London: Macmillan. 1954. Pp. 229. \$3.50.)

### Economic History; National Economies; Economic Development

APTKEKER, H. *The labor movement in the South during slavery*. (New York: Internat. Publishers. 1955. Pp. 26. 20¢.)

BERESFORD, M. *The lost villages of England*. (New York: Philosophical Lib. 1954. Pp. 445. \$12.)

A study of deserted villages: "... villages where we have clear evidence of their

- existence as communities in the Middle Ages: but where we now have no more than (at most) a manor house and a farm and a church" (from the introduction).
- BOURIEZ-GREGG, F. *Les classes sociales aux Etats-Unis*. Etud. et mém. no. 15, Centre d'Etudes Economiques. (Paris: A. Colin. 1954. Pp. 156. Fr. 850.)
- BRAKEL, W. *De industrialisatie in Nederland na 1945* (Industrialization of the Netherlands since 1945). (Leiden: Stenfert Kroese. 1954. Pp. viii, 175. f. 9.75.)
- BRAND, D. *Het streven van de economisch onontwikkelde landen naar een hogere levensstandaard* (The endeavor of underdeveloped countries to attain a higher standard of living). (Leiden: Stenfert Kroese. 1954. Pp. vi, 240. f. 12.)
- BUCHANAN, N. S. and ELLIS, H. S. *Approaches to economic development*. (New York: Twentieth Century Fund. 1955. Pp. xiv, 494. \$5.)
- CARTER, A. M. *The redistribution of income in postwar Britain—a study of the effects of the central government fiscal program in 1948-49*. Yale stud. econ. no. 3. (New Haven: Yale Univ. Press. 1955. Pp. viii, 242. \$5.)
- DAUPHIN-MEUNIER, L. *La cité de Londres et les grands marchés internationaux*. (Paris: Nouvelles Editions Latines. 1954. Pp. 358.)
- DE NEUMAN, A. M. *Industrial development in Indonesia*. (Cambridge, England: Students' Bookshops. 1955. Pp. 40. \$1.)
- DEPRIMOZ, J. *Les salaires et le niveau de vie ouvrier en Belgique (1936-1951)*. Etud. et mém. no. 14, Centre d'Etudes Economiques. (Paris: A. Colin. 1954. Pp. 204. Fr. 1100.)
- DIETERLEN, P. *Quelques enseignements de l'évolution monétaire française de 1948 à 1952*. Pub. of Centre d'Etudes Economiques. (Paris: A. Colin. 1954. Pp. 231. Fr. 750.)
- DINERSTEIN, H. S. and GORE, L. I. *Communism and the Russian peasant*; and II. *Moscow in crisis*. A RAND Corp. Research Project. (Glencoe: Free Press. 1955. Pp. xvii, 254. \$4.50.)
- EDELMAN, M. *National economic planning by collective bargaining. The formation of Austrian wage, price, and tax policy after World War II*. (Champaign: Inst. Labor and Indus. Relations, Univ. of Illinois. 1954. Pp. 78. Paper \$1.50, cloth \$2.)
- ERHARD, L. *L'expansion économique allemande*. (Paris: Domat. 1954. Fr. 975.)
- GALENSON, W. *Labor productivity in Soviet and American industry*. A research study by the RAND Corp. (New York: Columbia Univ. Press. 1955. Pp. xiv, 273. \$5.50.)
- HECKSCHER, E. F., translated by G. Ohlin. *An economic history of Sweden*. (Cambridge: Harvard Univ. Press. 1954. Pp. xlii, 308. \$5.)
- HUNOLD, A. C. *The industrial development of Switzerland*. (Zurich: Swiss Inst. for Internat. Stud. 1954. Pp. 45.)  
Also published in Fiftieth Anniversary Commemoration Lectures, National Bank of Egypt, Cairo.
- INGRAM, J. C. *Economic change in Thailand since 1850*. Issued under the auspices of the Internat. Secretariat, Inst. Pacific Relations. (Stanford: Stanford Univ. Press. 1955. Pp. viii, 254. \$5.)
- ISSAWI, C. *Egypt at mid-century*. (London: Oxford Univ. Press. 1954. Pp. xii, 389.)
- JORDAN, C. N. *The Romanian oil industry*. Published for the Mid-European Studies Center of the Free Europe Committee, Inc. (New York: New York Univ. Press. 1955. Pp. xv, 357.)
- JOSHI, N. S. and DHEKNEY, B. R. *Irrigation and agriculture in the First Five Year Plan—an appraisal*. I. (Poona: Deccan Book Stall. 1954. Pp. ii, 110. Rs. 6.)
- KUCZYNSKI, J. *Die Geschichte der Lage der Arbeiter unter dem Kapitalismus*. Vol. IV. Pt. I, *Die Geschichte der Lage der Arbeiter in England von 1640 bis in die Gegenwart*; I, *Vor der Industriellen Revolution 1640 bis 1760*; II, *Die Industrielle Revolution 1760 bis 1832*. (Berlin: Tribüne-Verlag und Druckereien des FDGB. 1954. Pp. 277; 247.)
- LOPEZ, R. S. and RAYMOND, I. W. *Medieval trade in the Mediterranean world*. Records of civilization, sources and stud. LII. (New York: Columbia Univ. Press. 1955. Pp. xi, 458. \$6.75.)

LOPEZ ROSADO, D., and others. *Problemas económicos actuales de México*. (México: Inst. de Investigaciones Econ., Escuela Nacional de Economía, Univ. Nacional Autónoma de México. 1954. Pp. 464.)

Papers by a group of Mexican economists on different aspects of the economic situation in 1953-54, with suggestions for policy.

MENDERSHAUSEN, H. *Two postwar recoveries of the German economy*. Contrib. to econ. analysis, VIII. (Amsterdam: North-Holland Pub. 1955. Pp. x, 130. \$3.25.)

MORAZE, C. *Les trois âges du Brésil*. Cahiers de la F.N.S.P. (Paris: A. Colin. 1954. Pp. 198. Fr. 650.)

PARRA, M. G. *La industrialización de México*. Colección Cultura Mexicana. (México: Imprenta Univ. 1954. Pp. 203.)

A study of Mexico's economic structure and recent industrial evolution, compared with that of the United States, presented as a reply to Professor Tannenbaum's *Mexico: The Struggle for Peace and Bread*.

RAPPARD, W. *A quoi tient la supériorité économique des Etats-Unis?* (Paris: Lib. de Médecis. 1954. Pp. 192. Fr. 450.)

ROMEUF, J. *Le niveau de vie en U.S.S.R.* (Paris: Presses Univ. de France. 1954. Pp. 140. Fr. 540.)

SVENNILSON, I. *Growth and stagnation in the European economy*. U.N. pub. 1954.II.E.3. Prepared by ECE. (New York: Columbia Univ. Press. 1955. Pp. xvi, 342. \$4.50.)

SWARUP, R. *Communism and peasantry. Implications of collectivist agriculture for Asian Countries*. (Calcutta: Prachi Prakashan. [1954.] Pp. 194. \$5.)

"This book tries to understand communism from an Asian standpoint. It tries to understand the ways in which communism is working out its strategy of domination of Asia, the slogans and sectors of population it uses in this process." (From the author's preface.)

TRIVEDI, A. B. *Post-war Gujarat—an economic survey after World War II*. (Bombay: A. B. Trivedi, Khalsa College. 1949. Pp. x, 289. Rs. 19/8.)

VAN DER VALK, H. M. H. A. *The economic future of Canada*. Originally published in Dutch. (New York and Toronto: McGraw-Hill. 1954. Pp. xiv, 206. \$4.75.)

WADIA, P. A. and MERCHANT, K. T. *Our economic problem*. 4th ed. (Bombay: New Book Co. 1954. Pp. xvii, 757. Rs. 8/.)

*El desarrollo económico del Ecuador*. U.N. pub. 1953.II.G.5. (México: Naciones Unidas, Comisión para América Latina. 1954. Pp. xvi, 218.)

An over-all survey of the Ecuadorian economy and its development possibilities. This study has not been issued in English.

*El desarrollo económico de México*. Cursos de invierno 1951-52. (México: Escuela Nacional de Econ., Univ. Nacional Autónoma de México. 1952. Pp. 272.)

Twelve essays by leading Mexican economists on various aspects of Mexico's economic development.

*Development of technical assistance programs*. Prepared by the Subcommittee on Technical Assistance Programs of the Senate Committee on Foreign Relations, 83d Cong., 2nd sess. (Washington: Supt. Docs. 1954. Pp. 129.)

*The economic development of Nigeria*. Report of a mission organized by the International Bank for Reconstruction and Development at the request of the governments of Nigeria and the United Kingdom. (Baltimore: Johns Hopkins Press. 1955. Pp. xxii, 686. \$7.50.)

*Foreign capital in Latin America*. U.N. pub. 1954.II.G.4. (New York: Columbia Univ. Press. 1955. Pp. vii, 164. \$1.75.)

*Ideology and reality in the Soviet system*. Proceedings of the American Philosophical Society, November 1954. (Philadelphia: American Philosophical Society. 1955. Pp. 38. \$1.)

Among the papers is one on "Economic Aspects" by C. B. Hoover.

*The Irish pound—1797-1826. A reprint of the report of the Committee of 1804 of the British House of Commons on the condition of the Irish currency*. With selections from

- the minutes of evidence presented to the committee and an introduction by Frank Whitson Fetter. (Evanston: Northwestern Univ. Press. 1955. Pp. 136.)
- Potential economic growth of the United States during the next decade.* By the Joint Committee on the Economic Report, 83d Cong., 2nd sess. (Washington: Supt. Docs. 1954. Pp. 35.)
- Rehabilitation and development of agriculture, forestry, and fisheries in South Korea.* Prepared for UNKRA by a mission selected by the FAO. (New York: Columbia Univ. Press. 1954. Pp. xviii, 433. \$8.50.)
- Report on the economic situation in Latin America.* Prepared for the Internat. Development Advisory Board. (Washington: For. Operations Admin. 1954. Pp. 216.)
- Report on the sample survey of Ceylon's consumer finances conducted in May, 1953.* (Colomba: Dept. Econ. Research, Central Bank of Ceylon. 1954. Pp. ix, 75. 50¢.)
- Toekomstige ontwikkeling van Nieuw-Guinea* (The future development of New Guinea). Vols. I and II. Government committee report. (The Hague: Staatsdrukkerij. 1953. Pp. 363.)
- Trends in economic growth—a comparison of the western powers and the Soviet bloc.* A study prepared for the Joint Committee on the Economic Report by the Legislative Reference Service of the Library of Congress. (Washington: Supt. Docs. 1955. Pp. xiii, 339.)
- "The purpose of this report is to summarize the essential known facts bearing upon the comparative rates of economic growth of the United States and independent Europe, on the one hand, and of the Soviet Union and the captive states on the other. . . . The study examines the changes in total economic activity, in the availability of resources, in structure and organization, and in the economic interrelations of these countries over the period 1938 through 1953. It indicates outstanding current problems and, where possible, the orders of magnitude of probable growth to 1970." (From the letter of transmittal by G. W. Ensley, staff director.)

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## NOTES

A nominating committee consisting of Ewald T. Grether, William Haber, Harlan L. McCracken, Aryness Joy Wickens, Harold F. Williamson, and Edwin G. Nourse, chairman, has submitted the following slate of nominees for 1956 officers of the American Economic Association:

**President:** Edwin E. White, University of Wisconsin

**Vice President:**

Richard B. Heflebower, Northwestern University  
Paul T. Homan, University of California, Los Angeles  
Fritz Machlup, Johns Hopkins University  
Ralph A. Young, Board of Governors, Federal Reserve System

**Executive Committee:**

Henry B. Arthur, Swift & Co., Chicago  
William J. Fellner, Yale University  
Walter E. Hoadley, Jr., Armstrong Cork Co., Lancaster, Pa.  
Richard A. Musgrave, University of Michigan

**Representative of Social Science Research Council:**

John P. Miller, Yale University

The annual meeting of the Association will be held at the Hotel Commodore, New York, December 28-30, 1955.

### PUBLICATIONS

The Graduate School of the University of the East, Manila, Philippines, began publication, in 1954, of a quarterly journal entitled *Economic Research Journal*, devoted mainly to economic problems of the Philippines.

*Yiddish Scientific Institute—Yivo.* The Commission on Research of the Yiddish Scientific Institute, 535 West 123rd Street, New York 27, N.Y., as part of its regular program, is compiling a continuing bibliography of social scientific studies in all aspects of American Jewish life. Scholars and communal agencies are requested to forward information about recent or current studies, published or unpublished.

### INFORMATION BUREAU FOR AMERICAN SCIENTISTS VISITING GERMANY

The "Atlantik-Brücke," a group of private citizens interested in furthering better understanding of Germany in the United States and vice versa, has established an Information Bureau. The bureau provides, particularly to visiting Americans, information on German research institutes, universities, libraries, scientists, their publications and their research in progress in the field of German social sciences. Interviews with experts on economic, social or political questions will be arranged on request. Communications should be addressed to the Information Bureau on German Social Sciences, Hamburg 13, Harvestehuderweg 9.

### Death

Victor W. Bennett, chairman of the department of marketing, University of Miami, died in March 1955.

### Appointments and Resignations

Henry G. Aubrey is on leave from the research department of the Federal Reserve Bank of New York to serve as economic consultant to the Harvard University's Graduate Faculty of Public Administration team of advisers to the Pakistan Development Board.

James A. Barnes, of Temple University, has been visiting professor of economics at the University of Miami during the academic year 1954-55.

Harold Bierman, Jr., has been appointed assistant professor of accounting in the School of Business, University of Chicago, effective October, 1955.

Gladys Boone, professor of economics and chairman of the Division of Social Studies at Sweet Briar College, has spent sabbatical leave during the second semester 1954-55 at the Institute of Industrial Relations, University of California, Berkeley.

George H. Borts has been promoted to associate professor of economics at Brown University.

Jess W. Brandon has been promoted from assistant professor to associate professor of accounting in the School of Business Administration of the University of Miami.

William N. Breswick has resigned from Southwest Research Institute, San Antonio, to become associate economist at the Phoenix office of Stanford Research Institute.

Donald J. Butterworth has been appointed associate professor of marketing at the University of Florida.

Burnham O. Campbell has been appointed acting instructor in economics at Stanford University.

Alan R. Cerf, of Stanford University, has accepted an appointment as assistant professor of accounting in the School of Business Administration, University of California, Berkeley.

Lester V. Chandler has been appointed chairman of the department of economics and sociology at Princeton University, replacing Richard A. Lester who is to be on leave next year.

H. W. Cordell, of Ohio State University, has been lecturer in marketing at the University of Florida in the second semester of the past academic year.

Virgil D. Cover, on leave from Syracuse University until September 1956, is visiting professor of Business Administration at the University of Rangoon, Burma, on a Ford Foundation appointment.

Richard M. Cyert has been promoted to associate professor of economics at Carnegie Institute of Technology.

Frank Dunbaugh has been promoted to associate professor of marketing in the School of Business Administration of the University of Miami.

Warren W. Eason, of Johns Hopkins University, has accepted an appointment as assistant professor of economics and bicentennial preceptor at Princeton University.

Corwin D. Edwards, visiting professor at the University of Virginia, has accepted an appointment as professor of government and business in the School of Business, University of Chicago, effective October 1955. In April of this year he delivered four Merrill Foundation lectures in Cleveland under the joint sponsorship of Western Reserve University and Case Institute of Technology.

Gertrud G. Edwards has been visiting lecturer in economics at Sweet Briar College in the past semester.

Denis A. Flagg has resigned from the department of economics, University of California, Berkeley, to accept a position at San Diego State College.

Edward J. Fox, of the University of Western Ontario, has been appointed visiting professor of marketing for another year in the School of Business Administration, University of Miami.

Heinrich Friedlaender, formerly professor of economics, Harpur College, State University of New York, is now consultant, Frankfurt/Main, Germany.

Eirik Furobotn has been appointed instructor in economics at Rensselaer Polytechnic Institute.

Charles C. Gersna, of the University of Michigan, has been appointed interim instructor of marketing at the University of Florida.

Frederick H. Harbison, of the University of Chicago, has accepted an appointment as professor of economics and director of the Industrial Relations Section at Princeton University effective September 1955.

George R. Hawkes has resigned as lecturer in the School of Business Administration, University of California, Los Angeles, to accept a position with the accounting firm of Price Waterhouse & Co.

Everett D. Hawkins, of Mount Holyoke College, was visiting lecturer in economics at the University of Massachusetts in the spring term.

William G. Heuson has been promoted from assistant professor to associate professor of finance in the School of Business Administration, University of Miami.

John W. Hooper has been appointed acting instructor in economics at Stanford University.

Marshall Howard, of the University of Massachusetts, has been visiting lecturer in economics at Smith College in the past academic year.

John Ise, who has been teaching economics at the University of Kansas since 1916, retires this year.

Neil H. Jacoby, who has been on leave to serve as a member of the Council of Economic Advisers to the President, has returned to his post as dean of the School of Business Administration, University of California, Los Angeles.

Myron L. Joseph has been promoted to associate professor of economics at Carnegie Institute of Technology.

Alfred E. Kahn has been promoted to professor of economics at Cornell University.

Eloise Kimmelman has been promoted from assistant professor to associate professor of accounting in the School of Business Administration, University of Miami.

E. A. Kincaid, of the University of Virginia, has been lecturer in finance at the University of Florida in the spring term.

E. R. Kuchel has been promoted from instructor to assistant professor in the department of economics of the University of Wyoming.

Leland C. Lehman, of Denison University, has accepted a research position with the Federal Reserve Bank of Minneapolis while on sabbatical leave.

Clarence D. Long has returned to Johns Hopkins University after serving with the Council of Economic Advisers as senior staff member in charge of labor matters.

Fritz Machlup, of the Johns Hopkins University, will be in Kyoto, Japan, until August where he is serving as visiting professor at Kyoto University and Doshisha University and as director of the Kyoto American Studies Seminar.

Edwin Mansfield, currently in England as a Fulbright scholar, will be assistant professor of economics next year at Carnegie Institute of Technology.

Julius Margolis has resigned from Stanford University to accept a position as lecturer in economics at the University of California, Berkeley.

Henry E. Mattox has been appointed instructor in economics at the University of Mississippi.

Alexander Melamid has been appointed assistant professor of economic and political geography in the Graduate Faculty of Political and Social Science of the New School for Social Research.

Hyman P. Minsky has been promoted to associate professor of economics at Brown University.

George Moss, who is at present on the accounting staff of the University of San Francisco, will return to the University of Miami as assistant professor of accounting in the fall.

John H. Mudie, of the University of Texas, has been appointed acting assistant professor of economics at Texas A & M College.

Eastin Nelson has been granted leave of absence from the University of Texas to accept a visiting lectureship in economics at San Carlos University, Guatemala City, Guatemala.

Ruth Bilsky Norr has been instructor in economics at Wellesley College in the second semester of the current academic year.

Robert T. Patterson, formerly of New York University, is joining the staff of Claremont Men's College as associate professor of public finance for the fall semester 1955-56.

Gaston V. Rimlinger has been appointed instructor in economics at Princeton University.

George W. Robbins has been named associate dean in the School of Business Administration, University of California, Los Angeles.

Merrill J. Roberts has been appointed professor of economics at the University of Florida.

Robert M. Robinson, of the University of California, Berkeley, has accepted a position at Sacramento State College.

Ralph L. Sackett has been appointed chairman of the department of marketing in the School of Business Administration, University of Miami.

Irving Schweiger has been appointed associate professor of business administration in the School of Business, University of Chicago.

Howard Shuman has resigned from the University of Illinois to accept a position as legislative assistant on the staff of Senator Paul H. Douglas.

Alfred G. Smith, Jr., has been promoted from associate professor to professor of economics in the department of economics at the University of South Carolina.

F. DeVere Smith has been promoted from associate professor to professor of economics in the School of Business Administration, University of South Carolina.

Victor E. Smith, of Michigan State College, has spent this year at Cambridge University as the holder of a faculty fellowship from the Fund for the Advancement of Education.

Thorn K. Snyder has resigned from Purdue University to take a position with the Arabian American Oil Company in Saudi Arabia.

Milton H. Spencer has resigned from De Paul University to accept an appointment as associate professor in the School of Business Administration at Wayne University.

Harry Stark has been promoted from lecturer to assistant professor of economics in the School of Business Administration of the University of Miami.

George A. Steiner has been granted a year's leave of absence from the University of Illinois to serve as senior economic adviser in the development planning department of Lockheed Aircraft Corporation, Burbank, California.

Herbert E. Striner has resigned from the National Science Foundation as study director for industrial research to accept an appointment with the National Planning Association.

James MacD. Terrell has been appointed instructor in economics at Brown University.

Proctor Thomson, formerly of the University of Chicago, is joining the staff of Claremont Men's College as associate professor of economic education for the fall of 1955-56.

Otto von Mering has retired from the department of economics at Tufts College.

Harry I. Warner has been promoted from lecturer to assistant professor of accounting in the School of Business Administration, University of Miami.

Harold L. Wattel has been granted leave from Hofstra College to serve as economic consultant to the Consumer Counsel to the Governor of New York State.

H. Lawrence Wilsey has been appointed chief of the China Division of the Foreign Operations Administration, Washington, D.C.

Alvin B. Wooten has resigned from Texas A & M College to take a position with the Texas Agricultural Extension Service.

James S. Worley has been appointed instructor in economics at Princeton University.

## VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

### *Vacancies*

*Private and public finance:* Ph.D. with emphasis in private and public finance for a large coeducational Catholic university in the Midwest, beginning in fall, 1955. Prefer man with some institutional and some teaching experience. P170

*Research in economics and investments:* Large Eastern company needs a versatile research analyst to work on a wide variety of problems in an established economic-investment research unit. Requirements: At least the M.A., M.S., or M.B.A., preferably Ph.D.; good background in economics with specialization in monetary, investment, or insurance fields; the ability to write well; age not over 35; good personal appearance. This is a research position, but the man must also be able to make a favorable impression on our line executives in both written and oral presentations of his research findings. P171

*International travel opportunity:* The Council on Student Travel invites applications for short-term employment as educational directors on trans-Atlantic ships. Required: educators from the following fields: cultural anthropology, art history, international relations and economics, group work, language, philosophy, recreation, sociology. Conversational ability in one or more of the following: French, German, Greek, Italian. Ability to organize extensive educational program with the help of passenger volunteers. From March to December, 1955, the program will be conducted by the Council for students, tourists and migrants traveling aboard two large passenger ships belonging to one of the major lines. These ships sail regularly from New York to European and Mediterranean ports. Job assignments vary from one round-trip sailing to periods of 3-5 months. Compensation for these longer periods of service will be regular salary. For a single round-trip sailing, full or partial passage, depending upon job requirements. All positions allow for time abroad. For further information, application form, write to: Council on Student Travel, (0-1), 179 Broadway, New York 7, N.Y. REctor 2-0936.

*Marketing, management, real estate:* School of Business needs assistant professors in above fields, beginning September, 1955. Terminal degrees preferred. School located in Midwest. In replying, please include personal history, education, and employment experiences. P173

*Economic principles and theory:* Instructor or assistant professor, Catholic co-educational college in large midwestern center. Master's degree required. Salary four to five thousand, depending upon the qualifications. No objections to outside work. P174

*Business administration:* Instructor or assistant professor, small Catholic co-educational college in large metropolitan, midwestern center. Salary four to five thousand depending upon qualifications. Should be able to teach management and general business subjects. Ample opportunity for consultation or other outside work if desired. P175

*Teaching position in economics:* The University of Puerto Rico has a position available for a young man, preferably single, having at least an M.A. degree in economics. Please write to Dr. William H. Beckwith, Director, Division of General Studies, College of Agriculture and Mechanic Arts, University of Puerto Rico, Mayaguez, Puerto Rico.

### *Economists Available for Positions*

*Industrial surveys, personnel management, market research, international economics:* Man, 36, Ph.D. Extensive experience with industry, government, advertising, foreign intelligence; various teaching positions; publications; supervisory, editorial, public relations, and lecturing experience. Several foreign languages. Presently employed in New York City headquarters of an international organization. Interested in more responsible position with industrial, commercial company or large university. E450

*Business cycles, economic development, public control, transportation:* Man, 35, M.A., Ph.D., University of Pennsylvania. Nine years of teaching experience at leading institutions; 5 years in industry. Broad familiarity with manufacturing and transport industries; good publication record. Desires teaching and/or research. E528

*Economic principles, labor, economic thought, economic history, economic systems, corporation finance, marketing, money and banking:* Man, A.M., Northwestern University, Ph.D., Yale University. Fifteen years of economics teaching experience; government experience as price economist. Broad social science background. Project in American economic field completed. Available in June or September. E536

*Economic and financial history, economic theory, public finance, money and banking, labor economics, corporation finance, investments and allied subjects:* Man, 40, married, Ph.D. Now teaching at large Eastern university. Interested in teaching position or perhaps a combination of teaching and some administrative work. E542

*Economics of the firm, public utilities, principles, value and distribution theory, public finance, government and business, business cycles:* Man, 33, married; M.A., University of Toronto, Ph.D., University of Wisconsin. Teaching since 1945; broad occupational experience, veteran, fellowships, articles in preparation. Desires teaching or position as economist for large firm. Available in June or September, 1955. E544

*Public finance, price theory, national income analysis, social control, money and banking, economic principles:* Man, 30, married; Ph.D., summer, 1955. Two years of experience in a large state university teaching mainly advanced courses; 2 years of experience as an economist specializing in the analysis of prices and national income for a research division of the federal government. Currently teaching. Desires teaching and/or research position. E545

*Labor economics, industrial relations, economic analysis, money and banking:* Man, 36, Ph.D., University of Chicago. Experience includes graduate and undergraduate teaching, top-level federal government position, university research, and industry. Interested in teaching and research. E549

*Economic history, business history, insurance history:* Man, 36, Ph.D., New York University. Fourteen years of governmental experience, including those of an advisory and administrative nature; 1 year of teaching experience. Has considerable counseling experience. Desires research, writing, or teaching position. Interested especially in college or university teaching position. Of especial usefulness to life insurance companies as research assistant or historian. E553

*Economics, statistics, corporation finance, investments, accounting, tax accounting, marketing:* Man, 40, married; B.A., M.A., Ph.D. requirements completed except the dissertation. Ten years of teaching experience. Desires teaching or investment position. E557

*Comparative economic systems, principles, corporation finance, public finance, labor, economic history, business law, constitutional law:* Man, middle age; M.A., LL.B., and course requirements in economics for Ph.D. recently completed at a Big Ten university. Ten years of legal and business and 6 years of teaching experience, including 3 years at university level. Is primarily interested in the stimulating type of undergraduate teaching. Desires a teaching position. Available in June or September, 1955. E558

*International trade, money and banking, investments, market research, labor economics:* Man, married; Ph.D. expected this spring. Three years of university teaching numerous fields in economics; 7 years of government research experience in cost and statistical analysis, interindustry relationships, price studies, and labor relations. Interested in teaching position or opening in private industry in management, sales engineering, or foreign trade. Available immediately. E562

*International trade, comparative economic systems, urban land economics, marketing, history of economic thought, public finance, money and banking, economic history of Europe:* Man, Ph.D. Research fellowships; 7 years of university and college teaching; 4 years of research in federal government. Experience as consultant; multilingual; extensive foreign travel. Desires teaching or research position. E563

*Economic theory, statistics, accounting, money and banking, private and public finance, mathematical economics, international trade:* Man, 35; B.S., M.A., Harvard, plus 2 years of graduate study on economic theory and statistics. Now assistant professor of economics and special lecturer of statistics; 6 years of successful college teaching experience. Other experiences include federal government position, university and industry research. Desires better teaching position or responsible statistical research position. E564

*Economic history, history of economic thought, economic theory, money and banking, business cycles, international trade and economics, economic geography, corporation finance, investments, marketing:* Man, 30, Ph.D., Rotterdam. Has some teaching, industrial, and marketing experience and excellent references. Desires teaching, research, or advisory position in California (San Francisco area). Arriving June, 1955. E565

*Marketing research, salesmanship, retailing, business cycles, money and banking, principles of economics, principles of marketing, labor relations, personnel administration, business organization and management, American industry and economic geography, foreign trade:* Man, 32, married; B.A., M.B.A., Ph.D. course and written requirements completed at New York University (Graduate School of Business Administration). President of small business 2 years; former assistant professor of economics at a large Southwestern college; 6 years of college and university teaching plus practical business background. Interested in position as teacher, in administration, or with industry. E568

*Labor economics, industrial relations, advanced economic theory, monetary theory, history of economic doctrines, sociological theory, methodology:* Man, 40, married, Ph.D. Twelve years of teaching experience on both the graduate and undergraduate levels; publications. Seeks teaching and/or research. Available in September, 1955. E570

*Public regulation of business, public utilities, transportation, communication:* Man, Ph.D. Teaching position desired in June or September, 1955. E571

*Investment analysis, auditing, budgeting, business finance:* Man, 51, J.D. Twelve years of experience in banking, credit and investment transactions; 5 years of experience in auditing large European organizations; 13 semesters of teaching experience as a professor in a fully accredited university in the U.S. Desires position as an investment analyst, controller, or treasurer. Also interested in teaching position. Available in June, 1955. E572

*Beginning economics, economic geography, money and banking, credit, retailing, marketing, public utilities:* Man, B.S., M.S., plus year of advanced study. Thirty years of experience in teaching and research work; 28 years of teaching in one institution. Being retired under faculty service ruling. Available for part-time, visiting, or supply instructor. Available in July or September. E573

*Economic principles, money and banking, public finance, corporation finance, investments, business cycles, international economics:* Man, 37, single; B.S., M.A., requirements for Ph.D. completed and dissertation in progress. Ten years of business experience in commercial banking and insurance underwriting; 2 years of teaching experience at outstanding midwestern schools; currently chairman of department of economics and business at a leading midwestern college. Desires a change to a better position offering greater opportunities for research and development. Stimulating teacher and public speaker. Partial to Southwest (Arizona) or West Coast location. Available in June, 1955. E574

*Economics of Far East, underdeveloped areas, international trade, money and banking, principles, price and income theory, cycles:* Man, 32, married; Ph.D. dissertation in progress. Eight years of teaching experience at state universities; 2 years as chief economist, stabilization agency; publications; journalistic background. Now teaching, but welcomes participation in research, teaching, or combination program with attention to Far East and underdeveloped economies. Available on short notice. E575

*Business research:* Man, 43, married; Ph.D. Industrial and university experience; publications; national convention speaker. Background in statistics, finance, economics, and business administration. Seeks either full-time research or research combined with teaching. E576

*Principles, corporation finance, investments, money and banking, American economic history:* Man, 56, married. Retired as officer of leading New York bank after 35 years of experience in investments and corporation finance; now completing studies for Ph.D. at Columbia University. Some teaching experience; extensive foreign travel; French and other languages. Desires full-term teaching position. E577

*Economic theory, international trade, marketing, comparative economic systems, public finance, investments, corporation finance:* Man, 42; Ph.D. Wide experience in teaching, high-level government service, and private industry. Interested in teaching, research and/or administration. E578

*Economic principles, comparative economic systems, labor, economic thought, economic history, corporation finance, money and banking:* Man, 29, married; Ph.D. course requirements completed. Two years of research experience; 2 years of teaching; now teaching in midwestern college; several European languages. Available in June or September, 1955. E579

*Economic theory and principles, history of thought, public and private finance, and other subjects:* Man, under 40, prewar Ph.D. Experience in banking, research, government service. Taught 17 years at every level through Ph.D. program, including most fields in economics and business curriculum, specializing in economic theory; past 7 years at large state university, with permanent tenure. Original contributions to scholarly and popular journals in economic theory, finance, and other fields; pioneering book to be published this summer; three others, including textbook, in progress. Minimum: full professorship. Please state salary. Available immediately. E580

*Economics, business administration:* Man, 51, married; B.S., M.A., near Ph.D., Chicago. Nineteen years of university and college teaching, much of the time as head of departments of economics and business administration; 5 years of responsible government work as economist, business analyst, or administrative assistant. Wide business experience, selling, sales management, accounting and tax fields; now associate professor of management, California college. Primarily interested in teaching position West or Northwest (preferred). Available in June or September, 1955. E581

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